

JUNE 1968 • 85 CENTS

BULLETIN OF THE ATOMIC SCIENTISTS

science and public affairs

The World Market
War and Taxes
The Great Evasion
War and Disease

David Inglis
Barbara Ward
Robert Eisner
Sidney Slomich



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JUNE 1968

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DAVID R. INGLIS

NUCLEAR THREATS, ABM SYSTEMS, AND PROLIFERATION

The destructive power of nuclear weapons is so terrifying that the most essential objective of our national nuclear policy is to avoid their use in all-out war. The purpose of our vast strategic striking force is to deter strategic nuclear attack against our cities. Our smaller but still very powerful tactical nuclear weapons were stationed in Europe to deter the Soviet Union from launching a land attack there. This is the great "nuclear umbrella" intended to prevent the outbreak of nuclear war.

Those who, in the early days of the atomic age, thought in terms of "one world or none" contended that with so terrible a nuclear threat all wars must cease, for a conventional war would too easily escalate into nuclear war. It was argued that the hard-pressed side, rather than admit defeat, would be sorely tempted to resort to nuclear weapons. Instead, the prevailing doctrine has been that the competitive pursuit of national interests in the real world will continue to lead to small conventional wars—"brush-fire wars" was the popular term—under the cover of the nuclear umbrella that should prevent their becoming nuclear.

We are now engaged in an oversized "brush-fire war" in Vietnam, a war that is alarming both in the extent of misery inflicted and the amount of national strength deflected from better ends on both sides. In it the intensity of the action has had its ups and downs. As this is written the exposed bastion at Khe Sanh is no longer a hot spot, but a few months ago it was looked upon as a potential Dien

Bien Phu, and the President took the unusual step of exacting from each of the members of the Joint Chiefs of Staff an assurance that it would not fall. In the tenseness of that situation—and a similar situation may arise again—there seemed to be genuine uncertainty as to whether we would use tactical nuclear weapons against the Vietnamese enemy in a pinch. The matter was discussed, by Senators Fulbright and McCarthy, among others, and when pressed the President would only say, negatively, that the use of nuclear weapons in Vietnam was not under consideration and he had had no requests for their use there. At about the same time General Wheeler said that he did not believe that nuclear weapons would be necessary for the defense of Khe Sanh. While it is gratifying that he turned out to be right and that nuclear weapons have not been used in Vietnam, his statement, coupled with the President's faint denials, reflected a fluid state of policy. Less prominent generals made more belligerent statements, while background briefings by U.S. spokesmen suggested that tactical nuclear weapons would lose their deterrent effect if decisively renounced and the Secretary of State objected to having so sensitive a matter discussed in public.

Our leaders know full well the importance of keeping inviolate that precious line of demarcation between conventional weapons and nuclear weapons, clearly marked as it is by the radioactivity of even the least powerful nuclear bursts. They know that escalation must stop at that line. They

know that our homeland is vulnerable only to nuclear weapons, and that it would be unutterably stupid for us to initiate their use in this age of nuclear competition. They know of the risk of bringing China and Russia into the war, and they know, too, that if we use tactical nuclear weapons they could be supplied to the other side, and that our great concentrations of supplies and troops would be ideal targets. Yet our leaders' faint denials inspire no confidence that in the anguish and heat of battle they would remember what they know.

But even if we assume that the Administration will not be so irresponsible and foolish as to use tactical nuclear weapons against Vietnamese, it is still a great mistake to leave the question open. The President's faint denial and the more direct suggestions by subordinates of possible nuclear strikes had the effect of being a slightly veiled nuclear threat against the Vietnamese enemy, probably an intentional one meant to "keep them guessing." This introduces an entirely new dimension into the strategy of the nuclear age, for it is employing a nuclear threat, not to deter attack by a nuclear nation and prevent the outbreak of a war, but to deter a non-nuclear country from some act of conventional local attack within a "brush-fire" battle already in progress.

In merely implying this threat, whether intended or not, the Administration is still being extremely shortsighted, irresponsible, and foolish. Very high on our list of national priorities, and properly so, is avoiding the proliferation of nuclear weapons to

of the effectiveness of our established posture of nuclear deterrence, which is directed toward nuclear nations. In this spirit there is a resolution before Congress, submitted by Representative Bertram Podell (D., N.Y.), that reads: "Resolved: That it is the sense of the House of Representatives that the United States should not use nuclear weapons against any nation not possessing such weapons."

Such a policy would tend to calm world tensions by de-emphasizing the role of nuclear weapons and removing their glamor value. It would also exert a more direct pressure on nations to remain nonnuclear. For a country in the throes of deciding whether to "go nuclear," the privileged status of nonnuclear nations, bestowed by this immunity from nuclear attack, would be a powerful argument in the hands of the political faction opposing acquisition of nuclear weapons. There have been negotiations with the USSR about the possibility of making jointly a "no use against nonnuclear nations" pledge to accompany the declarations by the nuclear superpowers guaranteeing the nonnuclear signatories of a nonproliferation treaty against nuclear attack. The rigidity of diplomatic niceties seems to inhibit our accepting a Kosygin formulation that would exempt West Germany, with our nuclear weapons on its soil, from a joint pledge. If we cannot do it jointly, it seems reasonable that we should do it unilaterally, and let the Russians make a unilateral pledge with their wording if they will.

THE TEST FOR THE NONPROLIFERATION TREATY

many countries. A future with many nuclear nations would be a diplomatic nightmare; in it the triggering of a worldwide nuclear war would be all too likely. The nonproliferation treaty negotiated at Geneva is an important beginning if enough nonnuclear nations can be induced to sign and stay with it.

Whether they will sign the treaty and long submit to its restrictions will depend upon the importance that is attached to possession of nuclear weapons. As long as nuclear weapons are just a part of the nuclear umbrella—a mutual threat by nuclear powers to prevent the outbreak of war among them—the nonnuclear nations may well feel glad to have no part of it. But now the foolish equivocal stance of our Administration had introduced a nuclear threat against a nonnuclear country into conventional warfare. For other countries this puts a premium on having nuclear weapons with which to face such a threat. In debating their adherence to the treaty, some of the most important nonnuclear nations have been calling for some step of nuclear arms restraint by the nuclear nations to match the restraint demanded of them. At the very least we should show enough restraint to clear up this matter and retract an implied nuclear threat against a small non-nuclear country.

Beyond that, it is imperative, in our national interest, that we should quickly proclaim a firm policy not to use nuclear weapons against any non-nuclear country. This would be a valuable step toward nuclear sanity and yet it would cost us nothing in terms

will come in obtaining adherence to the treaty by certain important "almost nuclear" nations, those capable of attaining nuclear weapons soon if they choose. The treaty cannot succeed in its aims without them. Some of them feel it a very real sacrifice to give up the option to make nuclear weapons under unforeseen circumstances in the future even though they now intend not to make them.

The reasonable and legitimate objections to the treaty arising from the almost-nuclear nations which appeared prominently in the discussions at a recent Pugwash symposium in London, center around the fact that the superpowers cannot stop their quarrels, and that in quarreling they cannot stop their arms race and even hide behind a dubious shield of ABM-type nuclear weapons while asking the almost-nuclear nations to renounce nuclear weapons altogether and submit to inspection in order to allay suspicion.

American promoters of an ABM system claim that it is oriented against China and that this should increase the credibility of a U.S. guarantee of China's neighbors. Leaders in those countries seem not to think this way, and this argument instead seems more importantly to enhance their impression of Chinese power. They see our defensive ABM shield as serving an offensive purpose because it could shield an American attack. They fear our ABM deployment because it helps to preserve our Cold War attitude toward China. For them it is important that the Chinese government evolve toward participation in the community of nations, and our ABMs push the evolution in the wrong direction.

Although the inspection and other safeguards are intended to be an advantage to the almost-nuclear nations by providing assurance about the behavior of their potentially nuclear neighbors, this aspect of it seems to loom less important in their eyes than the unjust asymmetry of the treaty, the asymmetry that makes it a treaty binding on the almost-nuclear nations, but a mere declaration of intent by the superpowers.

The declaration of intent, Article VI of the treaty draft, reads "Each of the Parties of the Treaty undertakes to pursue negotiations in good faith on effective measures relating to cessation of the nuclear arms race at an early

date and to nuclear disarmament, and on a Treaty on general and complete disarmament under strict and effective international control."

For including only this declaration, rather than, say, an obligation to forego ABMs and stop nuclear weapon production, the superpower argument has been that nonproliferation is urgent and it would take too long to fill in agreed details of such an obligation, so that the other nations must take the intention on faith until that "early date." In view of the sad record of superpower attempts at arms limitation and the recent plunge into the ABM-MIRV phase of the arms race, some lack of faith is understandable.

The intent so declared to seek cessation of the nuclear arms race is doubtless very sincerely held in those agencies of the superpower governments that have been most active in promoting the Nonproliferation Treaty, and most appreciative of the dangers of a future world of many nuclear powers. The question on which the future viability of the treaty hangs is whether these agencies can convince their governments to go along with some substantial measure of restraint for the sake of avoiding these dangers.

In this connection it is slightly encouraging to note that the votes in the Senate on amendments opposing the ABM deployment were considerably closer on April 18 of this year than a year and two years earlier. These were amendments to the military "authorization" bill and only preliminary to a later vote on an appropriation bill. The closeness of the votes encourages renewed efforts to block the ABM funding in the appropriation bill. It is a pity that a majority of senators still seem not to appreciate that our real problem now is not our relative strength vis-à-vis the USSR or China, which is more than adequate; not the stimulation of industry through defense spending; but far more importantly, the problem is the worldwide spread of nuclear weapons that we cannot stop without stopping the arms race by first stopping the ABM deployment.

The author is senior physicist at Argonne National Laboratory and a member of the Bulletin's editorial board.



THE NPT IS NOT ENOUGH FOR INDIA

INDU SHEKHAR MISHRA

The reported agreement on a nuclear nonproliferation treaty between the United States and the USSR is, as several other near-nuclear countries think, best suited to the interests of the two agreeing countries only. The hesitation of other nations to sign the present draft is based on genuine grievances which should not be ignored. The nuclear powers in this treaty propose not to give nonnuclear nations any nuclear weapons and at the same time deny them the right to produce nuclear devices. It is certainly a welcome step; but it is really doubtful if this treaty will ever be effective. I believe that the clause providing for international inspections by the International Atomic Energy Agency will never be truly implemented by potential nuclear powers or by other countries hoping to acquire nuclear weapons secretly from the big powers. Also, if the Soviet Union and the United States do not agree to such inspections in their own countries, how can they hope for such inspections in other countries?

If we are really interested in a safer world, we must first take effective measures to remove mutual fear and distrust among nations by following the principle of "live and let live."

Recently, the four big powers have been behaving in a way that has made developing countries count on their own strength and resources. Also there are many instances on record when the big powers did not honor their commitments to smaller powers, even during times of crisis. Therefore, if near-nuclear states feel that they cannot count on assurances of help from the USSR and the United States, in case of nuclear attack against any member state of the nonproliferation treaty by any other country, their hesitancy to

sign the treaty can hardly be questioned.

In developing countries like India, the UAR, and Pakistan, public opinion is so much in favor of going nuclear that it is difficult for the government to ignore the wishes of their people. For the past ten years or so, these countries, although economically underdeveloped, have been spending a considerable proportion of their national income on the development of a nuclear potential. Also, the large number of scientists in these countries who are involved in nuclear programs, have made the acquisition of a nuclear device a prestige issue. In regard to India, the general public as well as the scientists favor going nuclear. These sentiments have become so strong, particularly after the Chinese series of nuclear explosions, that until and unless the big powers appreciate the feelings of near-nuclear nations, any attempt on their part to effect a nuclear nonproliferation treaty is not likely to meet with success.

All well-meaning peoples in the world realize the dangers of a nuclear war and are sincerely in favor of a nonproliferation treaty. In fact, an effective treaty will be the first major step toward nuclear disarmament. Therefore, I quite agree with Bernard Feld ("To Be Nuclear or Not," May 1967 *Bulletin*) when he states: "The chances of obtaining meaningful measures of nuclear disarmament will be greatly enhanced by the achievement of a universal nonproliferation treaty at this time."

Professor Feld as a nuclear physicist reveals his sincere feelings toward the great goal of nuclear disarmament. I, too, as a physicist support his point of view which is, I am sure, guided by truth rather than by politics. Thus,

his discussion of the main arguments advanced against the proposed non-proliferation treaty—security, technological fallout, nuclear explosions for peaceful purposes, and industrial espionage—seem to be convincing when he describes them as: “fallacious or based on short-sighted political considerations.” Since India is more vulnerable to danger from the Chinese nuclear stockpiles than any other country, we naturally feel that a few nuclear explosions by us will neither minimize the Chinese threat; boost our security arrangements; nor afford us a major technological gain. The pretext that nuclear explosions for peaceful purposes should be allowed is a dangerous one; if scientifically analyzed it will be determined that it will do more harm to mankind than good. There are reasons to believe that nations who favor nuclear explosions for peaceful purposes will not be able to control their temptation to build up nuclear stockpiles for war purposes. The prime consideration of near-nuclear nations is that a nuclear explosion will be a great morale-booster for their military as well as civilian population. However, a few nuclear explosions are not in any way going to strengthen their security measures until and unless they develop an effective delivery system—which is unlikely since these countries do not possess the financial or manpower resources necessary for such an undertaking. The immediate result of nuclear explosions by these near-nuclear nations will only serve to escalate the nuclear arms race without any substantial gain to the countries involved.

I think these reasons should prove that Professor Feld’s statement—that arguments in opposition to the non-proliferation treaty are: “fallacious or based on short-sighted political considerations”—is not misconceived.

Why should the American and the Soviet governments promise the other nations of the world that after a certain period of treaty implementation, say two years, they will gradually dump their nuclear arsenal into the sea? Such a commitment on their part will assure the rest of the world of their sincerity in regard to nuclear nonproliferation. Also, the United States and the Soviet Union must find a way to include Communist China in this treaty; otherwise the nuclear

powers and, particularly, the Asian countries will never carry out their pledge to honor the terms of the treaty; they will secretly try to assemble greater nuclear capabilities to defend themselves against a Chinese nuclear threat. In that case, the treaty will be merely a treatise in white and black only.

Therefore, if the superpowers are really serious about nuclear disarmament as their goal, and about the nonproliferation treaty as the first effective step toward it, they should illustrate their sincerity and then approach the other nations in a frank and convincing manner. To achieve an effective treaty they must take into account the sentiments and fears of near-nuclear nations. If the treaty had been drafted and implemented by scientists and engineers of the concerned nations it would be a comparatively simple undertaking. It is unfortunate that the scientists and engineers who, in fact, are the creators of the nuclear age, who have done so much in the service of mankind, have no say in the most crucial issue that mankind has ever faced; but are exploited by selfish and power hungry politicians.

Dr. Mishra is a physicist in the department of physical research of the Central Institute of Fertilizer Technology, Fertilizer Corporation of India, Ltd., Sindri, Bihar, India.

The following response to Dr. Mishra’s article was written by Bernard T. Feld, professor of physics at MIT and president of the Council for a Livable World.

Professor Mishra is so obviously right that the only response possible is: “Amen!” Without firm commitments on the part of the nuclear powers—including, eventually, China—to submit themselves to the same controls as they demand of the nonnuclears, and to eliminate their nuclear arsenals, the nonproliferation treaty cannot last. Whether it will be politically possible for such nations as India to sign such a treaty now, in the absence of the required major-power agreements, is a question about which Indian scientists are in a much better position to judge and to act than their American colleagues.

—Bernard T. Feld

WHILE THE BUDGET WAS CUT, KIND WORDS WERE UTTERED

Senator Joseph Clark (D. Penn.):

“... the waging of peace will never come to an end; perhaps in our lifetime the waging of war can be brought to an end. Yet the Department of Defense is a permanent agency, and the Arms Control and Disarmament Agency [ACDA] is a temporary agency, whose lifetime has to be renewed every one, two, or three years, as the case may be. ...

“I hope we are moving toward a truce, and then a peace, in Vietnam, some kind of better understanding with the Soviet Union and other powers of the Communist bloc, and a very real opportunity for significant arms control and disarmament measures. ... and a treaty of general and complete disarmament under enforceable world law. I suggest the least we can do, if we do not wish to shatter such hopes, tenuous though they may be, would be to adopt this amendment and give the Agency a three-year lease on life and a \$33 million authorization.”

Senator Philip Hart (D. Mich.): “... when I consider the productivity, the results per dollar, that this Agency [ACDA] has obtained, it strikes me as a poor place to slow down the machinery. As to the committee’s proposed limitation on research funds for the Agency, I am reminded of a vote on the floor of the Senate last week. ...

“The Senate was asked to approve an increase in military R and D of more than \$500 million.

“The Senate should remember that the total military R and D budget was some \$7.9 billion—I repeat—billion.

“Now we are asked to find fault with the \$12 million that the Arms Control Agency had hoped to spend for its external research in fiscal 1969 and 1970. The committee recommends a limitation of \$7 million for this purpose.

“Mr. President, \$7.9 billion for military research versus \$7 million for arms control research! I am sure there are many pragmatic explanations, but might not a newcomer to this planet—if presented with these facts—be justified in wondering if our priorities were not a little lopsided?”

—U.S. Senate, April 22, 1968

IDA—A CONTINUING STORY

The following was written by Jane Wilson as a follow-up to her article which appeared in the May Bulletin.

On May 8 the University of Chicago announced that it was withdrawing from the Institute of Defense Analyses (IDA), an organization sponsored by 12 universities for the Defense Department. This was certainly no surprise. In March, the recommendation of the University's Academic Council, that Chicago end its sponsorship, was approved by President George W. Beadle as "responsible and sound."

IDA may no longer be an issue of the University of Chicago, but it is a very lively subject elsewhere. In response to unfavorable criticism on several campuses, triggered by the Students for a Democratic Society (SDS), the trustees of IDA resolved—in meetings on March 11 and 12—to modify the corporate structure and responsibility of the Board of Trustees by eliminating institutional membership altogether. Instead, the governing boards of each of the member universities would "designate annually a senior officer of their university to serve in an individual capacity as a trustee of IDA." This resolution must be approved by the member universities to be effective. The ratification of the resolution by the trustees of Columbia University was responsible, in part, for the crisis of Columbia in early May, when student riots disrupted the University. Student leaders demanded that Columbia's connection with IDA be severed completely. President Groyson Kirk, who serves on IDA's Executive Committee, refused to do so.

The trustees of Princeton University have also approved the compromise resolution, although a Faculty Committee had previously recommended withdrawal from the Institute. Citing the trustees' decision, among other grievances, 500 Princeton students marched in protest on May 2. In response, President Robert F. Goheen promised "a fresh and searching look at the University's decisionmaking processes."

—Jane Wilson

SPACE SCIENCE AND TIGHT BUDGETS

John Simpson, professor of physics and director of the Laboratory for Astrophysics and Space Research at the University of Chicago, testified before the Committee on Aeronautical and Space Science, U.S. Senate, on April 23. At issue was the funding for the U.S. space science program.

Professor Simpson argued that "The present situation is drastically reducing the momentum of science and the production of future scientific talent in the country." He called for every effort to reprogram funds so that "scientific enterprise does not perish."

Simpson described for the Committee the proposed pioneer space probes (missions for scientific observation conducted in interplanetary space and near the external magnetic field regions of planets) and the w10-foot radio antennas.

Among the most important experiments in space, he said, are "the observations of the charged particles—low-energy galactic cosmic rays, which stream into the solar system from interstellar space. They carry information crucial for understanding their origin and possibly the origin of elements. But the magnetic fields carried outward by the solar plasma tend to sweep them away from available points of observations deep within the solar system. However, there is the well-known 11 year solar activity cycle (the sun spot cycle), during which, at its minimum point of activity, the galactic particles come most readily inward—at least closely enough to study with Pioneer if the sun is relatively inactive. So there is an opportunity for a year or so of observation given to us by nature approximately once each 11 years to conduct these experiments under favorable conditions. Based on solar activity, our next opportunity is

estimated to be 1975-76, at which time the spacecraft would already have to be at approximately four to six astronomical units or more. Since to reach this distance requires approximately 2-2½ years of travel time, the launch from Earth would have to be underway by sometime in 1973. To meet this launch date, and recognizing the need for four or more years of development lead time, means that we must start this year if we are to have a good chance for success."

Professor Simpson concluded his remarks to the Committee with a plea for federal support of university-based research.

My emphasis upon increasing the support for research, especially at the universities, is based upon the importance of conducting sciences in the universities not only for discovery, but also for the growth of graduate students into scientists, and not super-technicians. It is our unshaken belief, on the basis of the development of science and scientists over the last century, that the drive and curiosity which motivate senior investigators within a university also provide the best intellectual environment for graduate students to learn and to be inspired by participation in scientific discovery. Indeed the two components—research and the metamorphosis of students into scientists—catalyze each other so that the result is a higher level of excellence than normally is achieved by the pursuit of either component alone. These activities are most fruitfully conducted within the walls of the institution of higher learning where the student is obtaining his higher education. This philosophy is also the main theme of the Seaborg Report of the President's Science Advisory Committee, and Nasa's program in earlier

years has strongly supported these principles. But as a result of present budget cuts and the choice of how the funds within Nasa will be used, the United States government has moved away from these basic principles. For example, last year the cut in the space sciences was approximately 27 per cent which represents a loss of somewhere between 50 to 70 per cent in terms of effective research results. Therefore it is urged to undertake a reassessment of this situation before our present course destroys the vital and creative parts of space science which have given it its unique character and leadership in the world of science. The flowering of first-class research and the reproduction of scientific and technical talent

is of a fragile nature—which is not easily measured by short-term rules of thumb.

"In closing, let me add one more thought, a deeply felt conviction: under the stress of the serious problems before our nation, in emphasizing the urgent we are in danger of unwittingly destroying what we wish to save and need, namely U.S. preeminence in science and technology which is crucial for each major problem of the nation, from poverty to war. I believe the present levels of support and the proposed programs are dangerously low. Therefore the actions taken in Congress this year may largely determine whether the United States will retain its leadership in the space sciences."

duce great benefits, of which by far the most important would be those that we cannot presently conceive of. That scientific forecaster saved his greatest contempt for what he termed the present-day doubters of the benefit of such technological undertakings. He contended that in future periods we all will look back with disdain upon these people as men of little faith.

To those who are neither scientific theologians nor wistful yammers for a simpler society, I offer a third position. It may be considered the agnostic view of the social scientist, and perhaps more particularly of the economist. To clear the air, I assume that we will not try to stifle scientific inquiry nor inhibit technological innovation. Also—and this may be the hooker—I assume that the determination of the uses to which public resources, particularly money, are put is a matter for the public to decide.

Hence, if a professor of engineering wants to devote his leisure time to designing a supersonic transport or planning a linear accelerator he will be entirely free to do so. However, the first time that he asks for \$100 million of taxpayers' money to start building the gadget, he will have to justify it—and not in the soft, theological terms of the natural scientist but in the hard, objective manner of the social scientist.

He will have to answer questions such as these: Are the expected benefits worth the cost? How well can he measure the benefits? Has he omitted important elements of cost to society, such as polluting the environment? Finally, and most crucial, are the returns from this use of public funds likely to be greater than from alternative uses?

We now expect such greatly maligned types as administrators of social welfare programs to make these calculations to support their budget requests for new training, health, and anti-poverty programs. I see great charm in extending the use of the scientific method to public resource allocation in the areas of science and, especially, technology.

The author is professor of economics at Washington University, St. Louis. The following was first presented to the Symposium on Science and Politics in a Democratic Society, University of Illinois, Urbana, March 7, 1968.

A MATTER FOR THE PUBLIC TO DECIDE?

MURRAY L. WEIDENBAUM

The great bulk of the public discussions dealing with the impact of science and technology on the United States is both discouraging and unproductive. On reflection, I think that this is so because the dialogue generally is limited to two polar alternatives.

The first polar alternative I would label the "view with alarm." It has become fashionable in many quarters, particularly in the humanities, to view with alarm the extent to which uncontrolled science and technology are destroying our society. Almost any issue of the Saturday Review, for example, contains another denunciation of these twentieth-century Philistines and their deleterious influence.

The second polar alternative is somewhat harder to define. It might be said that it looks upon science and technology almost as sacred cows—cows that can be milked however. Or it may be that the holders of this position do not really view science and technology as being beyond criticism, but, perhaps worse yet, as ends instead of means.

Hence, attempts by laymen to in-

volve themselves in science policy often engender cries of interference, shortsightedness, and worse. However, when I examine the actual justifications for undertaking new major scientific projects—development of a supersonic transport, construction of a linear accelerator, or penetrating below the earth's surface—I am always struck by the absence of that objectivity and hard, factual, quantitative analysis that I associate with the core of the scientific method.

I am amazed when scientists say that we must embark upon a major technological project on faith—faith that through serendipity (the invention of this all-purpose justification must rank as an important technological innovation in and by itself) it will turn out to be worthwhile after all.

Let me cite a case in point. During the past month, I attended an important meeting of a national scientific and engineering association. The audience was assured by one distinguished speaker that a specific current major technological undertaking would pro-

Barbara Ward is the Albert Schweitzer Professor of International Economic Development at Columbia University. She is a board member of the Adlai Stevenson Institute and a member of the Council of the Royal Institute of International Affairs. Her publications include *Towards a World of Plenty* and *The Rich Nations and the Poor Nations*.

The following article was presented before the Committee on Science and Astronautics, U.S. House of Representatives, January 23, 1968.

Technological Change and the World Market

BARBARA WARD

The world economy is the creation of technology. Technology in turn is the creation of two factors which first appeared in a decisive alliance inside the Atlantic community. One, of course, is experiment leading to invention. The other is capital or savings. But it is no coincidence that these two factors were first combined, on a scale sufficient to change the whole basis of production, in the market economies of Britain and then of West Europe. It is not much use inventing an article or a process and investing the money needed to perfect and produce it if, in the end, nobody wants to buy it at a price which covers the cost and leaves a margin to reward investor and inventor. True, war is a great forcing house of inventions, but military societies did not invent the modern economy. Nor is war the settled condition of merchant communities. Only when inventiveness began to be applied to all kinds of civilian needs—it all began, in a sense, with something as peaceful as textiles—did the technological revolution really get underway.

But it has not been evenly spread. The first technological revolution, based on steam power, iron and steel, and mechanization, produced one kind of pattern of imbalance between the various segments of the international economy. The second, rooted in electricity, the internal combustion engine, and chemicals, reinforced it. The new and still unfolding revolution of nuclear energy and the computer promises to lay new forces of disequilibrium on top of the old.

In short, one can say that the chief cause of imbalance outside the Atlantic area has been the relative failure of advancing technological change in the export sector of developing countries to stimulate decisive change elsewhere in the local economy. The reasons for this imbalance are inherent in the type of commercial exchange. Most of the capital for mines, plantations, power, transport, ports, and services was provided by the world's Atlantic "lead sector" where capital had begun to accumulate rapidly as a result of rising productivity, and where, for a time, the need for more raw materials became if anything steadily more acute. American and European enterprises provided most of the investment in wholly-owned subsidiaries—and the bulk of the management as well. Local earnings tended to be confined to unskilled wages and, in certain coun-

tries, suitable bribes to local politicians. Even this trickle of wealth was largely mopped up by the sale of imported goods shipped in by foreigners and often sold by large foreign merchant houses.

NEED FOR CAPITAL

From such an exchange, little local capital can be accumulated. And without capital, the essential technological transformations in other sectors cannot occur. Modernized education and widespread literacy, food production for the market as opposed to subsistence, industrial development to replace imports and upgrade local materials—all such programs require large inputs of capital. They require more—a vigorous, local, entrepreneurial middle class. But such men are not produced in large quantities by a trading system whose management and skilled leadership tend to be foreign. Where, as in most of Asia and Africa, government itself was also foreign—in other words, colonial—the political authority was not designed to redress the balance and compensate for the market's failure to provide general economic stimulus. Colonial governments did not tax expatriate firms and divert some of the profits to local development. There was usually no tariff protection for local industrialization—India had to wait until 1920 for a textile tariff. For this kind of economy, the distinguished French economist, Pèrre Lebret, coined the term *l'économie de traité*, the "milk-cow economy." And indeed if one looks at maps of North America and Europe and then of South America and Africa, the picture in the South with all the lines of communication running down to ports—which are almost the only cities—when compared with the interconnected spider web of transport and wide distribution of cities in the North, does give one the sense of looking at a kind of drainage system in which all the lines run to the sea while, in between them, there is little physical evidence of growth and change.

It is certainly true that, since 1947, the world market as a whole has undergone an expansion unequaled in scale and length since the mid-nineteenth century. World trade has grown, year after year, by not less than six per cent. The liquidity to sustain it has been provided by America's readiness to invest heavily overseas,

ment and the scale of the economy. Each is new. Each has an inhibiting effect on modernization. Each seems resistant to "normal" commercial solutions. Each raises again the issue whether pure reliance on the market is enough for successful technological change.

POPULATION AND RESOURCES

run a dollar deficit, and convert it into gold. Sustained growth rates of between three and four per cent a year in the Atlantic group of states have underpinned this steady maintenance of market demand. If the market alone were capable of pulling the whole world economy upward in the wake of Atlantic growth, then the last 20 years should have been triumphant proof of this. What in fact do we find?

Not, certainly, any evidence of complete failure. On the average, developing nations have managed to grow by 4.8 per cent a year, which is higher than the Atlantic average in the nineteenth century when boom and bust pulled down the overall level.

Saving rates in the developing world have risen, again on the average, to some 15 per cent of GNP. About 80 per cent of the capital devoted to development has been provided locally. One reason for this change is that with the approach and coming of political independence, most governments in developing countries have started to tax business enterprise and negotiate more favorable terms for mineral or land concessions.

All this is very far from the stagnation of the thirties. But equally it does not seem to have been enough to break completely with the old "milch-cow" pattern. The developing nations are still primarily exporters of raw materials; they account for only five per cent of the world's trade in manufactures. They are still over-dependent on one or two lines of export. They still lack that diversification of their economies—in agriculture, in industry, in education, in services—that alone can promise a full technological breakthrough.

The situation seems to be that on top of their old lopsided structure of change and growth confined largely to the export sector, the developing nations now face further inhibitions simply because they are trying to complete the process of modernization; not in the nineteenth century, but in the second half of the twentieth. Here we can pick out three such complications which nineteenth-century developers simply did not confront at all. The first concerns the balance between population and resources, particularly resources in food; the second the development of a dangerous rural-urban disequilibrium; the third the relationship between develop-

In the early nineteenth century, more productive agriculture and the beginnings of industry preceded modern medicine, public health, and bursts of growth in population. In fact, a growing population alone gave Britain the "hands" needed in the early days of mass unskilled labor. Only very large families and massive immigration provided America with the manpower needed to open up a continent. But Asia was crowded 2,000 years ago. India and China had over 150 million inhabitants in 1789 at a time when the United States had five million in an empty continent. Since then, public health and the control of epidemics have been introduced long before general modernization in either farming or manufacturing. The per capita levels of feeding may well have started to fall in India by 1900. A ruined, bankrupt countryside was one reason, among many, for Mao Tse-tung's triumph with the peasants of China. An annual spurt in population of 12 to 15 million a year in a country not yet fully modernized means quite simply that a four per cent rate of growth is swamped by a three per cent rise in population, leaving almost no margin for further investment and hence quite insufficient capital for the technological breakthrough. Such a lack in agriculture can mean deepening malnutrition and, in a bad year, famine.

Modernization in farming is impeded by the second inhibition—the imbalance between urban and rural development. Today, in the developing world, every vast coastal city, built in advance of industry to serve the export sector—Rio, Buenos Aires, Lagos, Bombay, Calcutta, Singapore, Manila—is a target for rural peoples pouring out of stagnant countryside before farming has enough technological capacity to afford to let them go or the city enough urban employment to receive them. Squatters move in like a flood. The figure for Rio, for instance, is more than 5,000 a week. They offer totally unskilled labor to industries which, to compete at all with Atlantic imports, seek to push forward with greater mechanization. A 60 per cent unemployment rate for young people is not unusual in the world's urban ghettos.

The question of urban-rural balance in employment is in turn bound up with the inhibition imposed by unsolved problems of scale. The developing countries are not entering a world market in which their standards and capacities set the pattern. They enter into competition with economies enjoying a century of industrial experiment and development largely based upon costly research and high capital investment. Mechaniza-

"Technological advance is essentially a matter of cultural development, a balanced mixture of ideas and idealogies which favor a more material way of life. Technology responds to markets, and markets reflect the tastes and desires of the people. . . . In essence, the point is that in today's mobile age, with consultants crowding the airlines, technical knowledge can be imparted into a region with little difficulty. What cannot be imported is the culture of technology. This must grow from within the community itself. The challenge and excitement of research and development activities, made more real through local involvement, are splendid levers to cause the process of technological advance to begin."

—Martin Golond, Committee on Science and Astronautics, January 24, 1968

tion means doing without some labor, computerization without even more. No doubt it is an advantage to have the technologies already invented and borrowable, but it costs scarce capital to install them. Then they mass-produce and this means a large market to absorb what they yield. Stagnant countryside and unemployed urban masses do not supply such demand at home.

PLIGHT OF EUROPE

The point can perhaps be illustrated more vividly by looking not at the developing lands, but at the contemporary plight of Europe. Here, too, economies confront a problem of scale. The first decade of the European Economic Community has shown that not European so much as American firms are taking advantage of the new continental scale. Accustomed to vast enterprises, they possess the managerial and technical skills needed to operate in Europe's new 200 million market. Europe cannot hope to compete with this new phenomenon of extensive American penetration of the new European market unless it is prepared to change a Common Market that is not much more than a free trade area with highly protected peasant agriculture into a genuinely unified continental economy whose European companies can work unrestricted by traditional legal and political obstacles and can draw on a new readiness in Europe to accumulate its own capital and invest in its own technological research.

If West Europe, after a hundred years of modernization and with one of the highest standards of living and learning in the world, can suddenly be confronted with critical problems of scale, it can be understood that the new mini-states of Africa or Southeast Asia or the traditional nationalism of Latin America are even more inhibited by the need to modernize at a time when, in so many technological fields, the relevant size of operation is not the nation, but the continent.

Are we to conclude that this combination of new obstacles piled on top of the old unbalanced nineteenth-century structure means an end to any hope of technological transformation for about half the human race? If this question means: can nothing more be done? the answer is clearly, of course an immense amount can be done. If it means: can this transformation be carried out by the almost undirected working

of the international market economy? the answer is, probably not; but this does not mean that there are no other methods. Let us look at the two questions separately. First, then, what sort of policies might actually be pursued to speed up technological change and modernization?

We can take three examples, each of central importance in countering the three major obstructions to future growth. The area of agriculture is in many ways the most hopeful because it is here that science-based productivity has made some of its most startling advances in recent years. Let us look at one of India's most backward states in the midst and aftermath of a narrowly averted famine. In the Kosi district of Bihar, as a result of 200,000 acres of land coming under irrigation in 1966, the newly stabilized high yielding grains—strains that were unknown even five years ago—could be introduced at once among them Taichung paddy and hybrid maize. The results have been fantastic. The output of paddy per acre has increased from 650 pounds to over 4,100 pounds, maize from 820 pounds to 4,000 pounds. It is figures such as these that underlie the growing confidence in India that this year's harvest of at least 95 million tons of grain represents not simply a better monsoon, but a much wider application of all the main technological improvements in agriculture, a new combination of water, fertilizer, and improved seed, yielding four and five times the old output. And such increases are enough to put food production well ahead of population growth. It is, incidentally, at this point, when children survive and living standards begin to edge upward, that policies for extending family planning begin to work. Development and family planning are the Janus-head of a single process of modernization. It is unrealistic to pursue them separately.

Revolutionary changes in agriculture are not, of course, confined to India. They are the basis of the most productive peasant agriculture in the world, Japan, followed closely by Taiwan. They can be found in Pakistan where a comparable revolution of water, fertilizer, and better prices to the farmer has doubled the rate of agricultural growth in the sixties. They reappear in East Africa in the land settlement schemes of Kenya. Wherever the farmer has the market inducements to produce and sell, he will run up a black market in

fertilizer before the planners have time to turn around. It is no accident that Japan leapt into modernization by way of the total abolition of feudal dues.

A further consequence of the technological revolution in agriculture is its impact on the second great problem in developing lands, that of devising and financing enough employment to absorb the growing population. The point of departure is the increase in farm income. We can take a typical farmer in India. He farms five acres of land and his net income at today's prices, using traditional methods and producing about 1.5 tons of grain, would be about \$67.10 a crop. With new methods—water, fertilizer, improved seeds—this output increases to 7.5 tons. Even allowing for higher costs, his net income still rises to \$400. If he can double-crop, it may double again. Thus he begins to follow the pattern in Japan where between 1870 and 1914 farm output and income tripled. The result was a surplus to transfer to capital for industry. Yet the rise in farm income was still sufficient to buy up the products of industry.

Unfortunately, in later efforts at development, Japan's example of the intimate link between agricultural reform and overall development has been somewhat overshadowed by other models—Russia's emphasis on heavy industry, America's industrial supremacy. Agriculture received less than its share of capital and attention. Equally, however, the new hybrids had not yet been adapted to local use. Perhaps a big agricultural effort in the fifties might have had less spectacular results and led to disillusion. What is certain now is that the sensational technological advances in agriculture coupled with farm-based industries permit a new and much more hopeful approach to the whole problem of general modernization.

A new emphasis on agricultural modernization and rural-based industry will also make some contribution to the third problem—the problem of scale. Here we leave India which, if it can preserve its political unity, has the continental scale needed for full modernization. But in much of Africa and Latin America, mini-markets stand in the way of satisfactory growth. However, it is precisely the larger and more spectacular acts of industrialization—the steel mills, the large smelters, the automobile assembly plants, the refineries, the petrochemical complexes—that demand larger economic *lebensraum* if they are not to make the depressingly frequent contribution of running at a loss and thus contributing to the country's underdevelopment. If the glamorous central theme of modernization could be seen to be transformation on the farms coupled with lively farm-based industry, governments agreeing to a national regional distribution of large units need not feel that they were losing out altogether on industrialization. They would still be growing just as fast as their neighbors and could continue growing while the tough bargaining on regional distribution of the larger enter-

prises continued. In the chequered story of East Africa's on-again, off-again Common Market, the present readiness to try again has coincided with a new realization of what land settlement and agricultural development can contribute to modernization.

RELiance ON THE MARKET

The various inhibitions to more rapid growth can be overcome. But when we turn to the second question, whether the forces working through the international market are of themselves sufficient to complete the task, the answer is much less certain. There are at least four reasons for doubting whether the more hopeful prospects can be realized by "normal" methods alone. The first is that the present 4.8 growth rates in the developing world have, in fact, been underpinned in the sixties by sustained economic assistance of the order of \$5 billion net each year from all sources. Some of this has been in grants, more in various forms of concessionary loans. The total inflow of financial resources has been nearer \$9 billion a year—a figure which incidentally includes grants and purchases of arms whose impact on development can be dubious, to say the least. But if the full revolution in agriculture is to be realized, President Johnson's scientific advisers suggest that an additional \$4 billion a year must be spent by the 1970s for fertilizer, improved seed, and pesticides. One should presumably add as much again for the whole infrastructure of water, power, farm-to-market roads, and farmers' credit. Even if cuts are made in less necessary investment, with arms, no doubt, at the head, these estimates imply a sizable increase in external assistance. Yet, at the moment, who would like to forecast such a possibility? On the contrary, the likelihood is for a general slash in aid in the wake of America's drastic axing of its program. It is true that 80 per cent of the investment underlying development has been produced locally. But the 20 per cent from abroad represents the critical element of foreign exchange which, at this stage, is indispensable for such purchases as fertilizer or, for the time being, food.

This fact brings us to the second reason for not relying on the market alone. Foreign exchange on a sufficient scale cannot be borrowed or secured through foreign investment. With a debt of over \$40 billion and obligations of repayment that threaten to swallow a third to a half of their foreign exchange earnings by the 1970s, developing countries cannot borrow much more. Nor do the bulk of them attract "normal" foreign investment. Nations with a per capita income of less than \$150 make up more than half the developing world. They receive less than 15 per cent of all foreign investment on commercial terms. The World Bank has had to virtually invent the International Development Association to supply the urgent needs of the poorest nations. And today the Association is empty and having some difficulty in securing replenishment.

Foreign trade is no alternative. Here we reach the third reason for doubts about the efficacy of "normal" means. The pattern of world trade has not yet changed in any decisive way since the bias against developing countries was imparted to it long before World War II. Tariffs on made-up goods in contrast with free entry for raw materials still discourage diversification and industrialization locally. The instability of primary prices continues. So does the pressure of substitutes invented by superior Atlantic research. Atlantic liberalism and generosity can hardly be said these days to be giving out a beacon light of inventiveness and progress.

This brings us to the fourth reason for doubt. Hitherto, growth and development have been sustained by the relatively smooth functioning of the Atlantic economy. Today, the richest member, America, is being forced to curtail its lending and spending abroad. As a result, the liquidity underlying the expansion of world trade may be about to be turned off. West Europe, where the large surpluses lie, is not likely to respond with greater spending itself. Restrictive reaction all round may be to cut aid, lessen free trade, and fail to put through the reforms in world liquidity which would be the creative answer to the disappearance of the American deficit. If all these restrictive possibilities are realized, the world economy will not face another decade of growth.

SUSTAINED TECHNOLOGICAL GROWTH

Clearly, the chief policies needed to sustain full technological growth are simply the obstacles turned on their heads. On the side of the developing countries, they include a new dedication to agricultural modernization, in both structures and techniques, emphasis upon the rapid growth of farm-based, consumer-oriented industries in regional urban centers, and the search for larger supranational free-trade areas or common markets within which the more complex and sizeable industrial units can find adequate demand for their products. On the side of the developed countries, the need is to realize the transfer of one per cent of GNP in genuine aid vaguely promised by rich governments to the poorer lands on various occasions; to sustain and expand the flow of private investment to the developing world; to put through such well-known and exhaustively considered reforms in foreign trade as more stable primary prices, preferential access to Atlantic markets for materials and manufactures as well as capital both for diversification and, as insurance against short falls in export income, a share in any new scheme of liquidity evolved by the wealthier powers. This agenda has been canvassed and re-canvassed continuously throughout this "Decade of Development." Only hitherto, like a mirage, when examined more closely, it always fades away.

Why is this? It cannot be, in any objective sense, a matter of resources. The combined national income of the Atlantic world passed the \$1,500 billion mark

last year. Since a three to four per cent annual rate of growth is now normal, this income increases by some \$50 billion a year, about 75 per cent of it in the United States. To dedicate one per cent of this rising income to aid means a slight sacrifice of the speed of future Atlantic growth. To devote, say, another one per cent in private investment is not a sacrifice at all, but a later addition to Atlantic wealth. The only reason why more ambitious policies for world growth are "impossible" in any sense today is that other priorities have greater importance, such as defense or rising domestic standards of living. (Ten cents in every dollar spent on Atlantic alcohol and tobacco would largely cover the bill for economic assistance to poorer lands.)

But the reason for the failure of any long term Atlantic strategy to emerge has, of course, nothing to do with resources. It is rooted in something much deeper—the manner of thinking about the world still dominant in our national societies. If the world were as small in our minds as it is in fact under the feet of the strolling astronaut, the imbalance in the planetary economy between ourselves—the 20 per cent with 50 per cent of the wealth—and everyone else would lead us to adopt the reforms we already accept, almost without question, inside our national societies. Between one per cent of GNP in assistance and our domestic income tax, there is no difference in principle, only in context.

The Atlantic powers have less good will and cohesion than they had five years ago. As a result, they lack the grip and energy to confront the broad problems of the planetary economy in which they have to live. But the lesson of their domestic economy is inescapable. The market is the prime mover in economic and technological progress. The Soviets themselves recognize this fact. But it is a market corrected and enhanced by policies making for balance, welfare, and the common good. At present, the world economy is trying to get on without these policies. As a result, in spite of all the brilliance of our scientific discovery and technological innovation, its problems, imbalances, and distortions are tending to grow worse.

We shall not reverse this trend by inaction. On the contrary, drift is carrying us on toward the rapids. Our need is rather, as Mr. George Woods has proposed, to call on the best experts to advise our governments and help them work out the implications of the world's dilemmas, to assess the scale of necessary action, weed out the failures, underline the successes, and devise a joint strategy for the future modernization and progress of our interdependent world. Only if we are ready for something like the sustained practice of welfare and justice we accept inside our domestic economy, are we likely to make much progress in the larger task of modernizing and domesticating the whole planet. Yet if we cannot do so, we must face the fact that the technology which might have united and enriched our world is much more likely to blow it up.

"Economists . . . should look for problems in society and not merely wait for the questions that are asked them, for indeed a biased selection of questions is likely . . . to keep us much further from truth than biased answers. And, if they are to maximize their value as economists, in the final analysis they should, extreme as this may seem, be loyal to no leader or party but rather strive with the skills they have and should always seek to improve, to be true to themselves, their profession, and the world in which they live."

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Sophisticated analysis of international relations frequently invokes a game: theoretic structure-Quemoy and Matsu; Cuba, Berlin and Budapest; Southeast Asia and its dominoes; the Middle East and oil and Israel; Latin America and the Alliance for Progress; Nato; the Common Market and de Gaulle; liberty, democracy, capitalism, and communism—all are costs or elements in pay-off matrices of a grand game, however deadly, involving us and our real or imagined foreign adversaries. Into the giant calculator go thousands, or millions, and hundreds of millions of lives, and, of course, tens and hundreds of billions of dollars; and outcome probability-weighted utilities—for "us" and "them."

In viewing the war in Vietnam and the associated economic measures for which the Administration so long solicited support, I should like to retain a game-theoretic approach. But I should urge one fundamental change in our view of the game. For the adver-

saries are not essentially the United States and the USSR, or the United States and China, or democrats and communists. Here I am not challenging the old monolithic view of "world communism" which, however anachronistic, has carried us so far into our current impasse. It is a concept of a monolith that I would challenge—but it is the view of Us as a monolith. For the simple, brutal fact, as I see it, is that national leaders and, in the first instance, the political leaders of the United States, should be seen perhaps contrary at times to what they themselves believe, as playing the game primarily in their own real or imagined interests, and not those of the people whose destinies they so greatly control. The American people become the forces to be manipulated, the instruments of some operation of their leaders, which may, almost by accident, happen to be in their own interests, but may just as likely have nothing to do with them. And, I shall point out, the same may be said for the political scientists and economists who think to guide and advise this Administration in achieving its objectives.

Each step taken by the President of the United States is calculated most importantly in terms of its effect, not on "Peiping" or "Hanoi," in the quaint jargon of our Secretary of State, but on the American people. It is they who are the chief aims of strategy. It is they who must be made plausibly belligerent and yet sufficiently malleable to fit the shifting tactics which may seem appropriate. And above all, the American people must be kept in a position where the leaders can continue to play their game, in their own interests. Where events make this no longer possible, certainly withdrawal from leadership becomes the alternative to political or physical extinction. For if ever the American

War and Taxes: The Role of the Economist in Politics

ROBERT EISNER



people find the game not worth the candle, then all is lost—not for the people themselves, but for those who are playing the game. For their function in life, what has come to be their *raison d'être*, is playing the game. If the game is taken out of their hands, they have lost all.

I am reminded of the slogan of some currency a few years ago: "Better Red than Dead!" This was denounced as an extreme leftist or perhaps coward's view of an appropriate preference function. Given the way popular prejudices are exploited, it was hardly a wise political slogan and I do not know that many dissenters, at least "responsible" ones, really advanced it. But was it so unreasonable? Or does it perhaps describe the revealed preferences of most people of the world, Americans not excepted? When Czechoslovakia "fell to the communists in 1948, Jan Masaryk was reported to have committed suicide. But [even if he did commit suicide] he was one of the leaders playing the game. Not many ordinary Czechs or Slovaks, to my knowledge, exercised a similar choice. The picture seems strikingly similar in East Germany, China, and Cuba.

PLAYING THE GAME

But here we are talking of preference functions of people, not leaders. Dulles was ready to run a small risk of nuclear war to prevent Quemoy and Matsu from falling to the communists some years ago. If the American people had understood the meaning of probabilities and the game being played, would they have been willing to run that risk, even if it were .01 or .001? Imagine a game that your typical American is offered the chance of playing to save Quemoy and Matsu. If he does not play, Quemoy and Matsu are "lost." If he does play, he faces a roulette wheel with integers running consecutively from 1 to 100. If the needle stops at any number from 1 to 99, Quemoy and Matsu are saved. But if the needle stops at 100, he and his family are dead! Will he play?

If that game seems far-fetched, ask yourselves what game was being played, and on whose behalf, during the Cuban missile crisis of 1962 when, according to Theodore Sorenson, referring to the late President Kennedy, the "odds that the Soviets would go all the way to war . . . seemed to him . . . 'somewhere between one out of three and even.'" (Kennedy, New York, 1965, p. 705.) Can that mean that the prompt removal of the missiles as requested by the U.S. government was considered worth a one-third or .5 probability of nuclear war? For whom?

But once we recognize that preference functions of leaders and led are not identical and that the leaders can affect the preference functions of the led, we look for explanations of behavior in terms of how that behavior is calculated to influence the preference functions of the led, and only hence the opportunities for decision and action by the leaders. Thus, to call the war in Vietnam "aggression from the North" is better

calculated to win support from the American people than to make clear, let alone emphasize, that we are involved in a civil war in a far-off land or, perhaps worse, a war against all of the Vietnamese, North and South. Then, however, bombing North Vietnam becomes important, regardless of any military considerations (or perhaps even if it is militarily counterproductive) because it is part and parcel of the strategy of the "game," not against the "enemy" but against the American people. To give up the bombing is to suffer a shattering defeat in the effort to capture and retain the support of the American people. A complete and unconditional end to the bombing thus assumes great significance far beyond its military import, denoting the giving up of the home political base for prosecution of the war.

THE ROLE OF THE EXPERT

What then is the proper role of a political scientist or an economist called upon to pronounce himself on matters of policy, whether ends or ostensible means regarding international relations—in particular, the war in Vietnam? Should he accept the stated ends of policy and confine himself to prescribing the technically most efficient means of realizing them? Or should he construe his role more broadly, and insist upon exposing to expert view and, where feasible, to public view, what might appear to him to be a more adequate structuring of the issues? If he perceives that the real issue involved in bombing North Vietnam is not stopping aggression from the North or saving the lives of American boys, but that of stilling criticism at home and saving American political lives, he should say so. He should insist upon setting the problem in the terms that are meaningful to him. To confine one's self to answering obediently the questions asked by the Administration may be to bias the results and prostitute one's professional skills to the service of misleading the public, no matter how "honest" the right answers to the wrong questions.

As you may gather, I believe that economists have much to offer in the understanding of political affairs. Aside from the dominant role of economic considerations in any society, what is involved here is the usefulness of modern tools of economic analysis—the application of the whole paraphernalia of constrained or unconstrained maximization of preference functions, game theory, and even the tools of econometrics—to estimate parameters of the critical behavior functions of political affairs. (What, for example, really is the derivative of infiltration with respect to bombing raids in Vietnam? Is it perhaps possible that there has been more infiltration rather than less as a consequence of the bombing?) But having raised some of these matters more narrowly in the political domain, I might wisely retreat to what is probably for me more familiar ground; the economics and politics of certain aspects of our fiscal policy, particularly as they relate to the war.

Having gotten actively involved in the controversy

over the proposed income tax surcharge and associated tax increase I have some rather strong views on the matter, which I might as well express.

Let me quickly set forth a few of the axioms of the New Economics, of which I am a fairly faithful disciple and proponent. First, we hold that full employment is not a necessary state of a capitalistic, "free enterprise" economy. Second, we hold that measures of monetary and fiscal policy (particularly fiscal policy, that is, government spending and taxing) are appropriate to raise the expected level of employment. Third, we recognize that more government spending, other things including tax rates being equal, will mean higher employment but more chance of price increases, or the same chances of greater price increases. Fourth, we recognize that, other things including government spending being equal, higher income tax rates will mean less private spending and hence a lower level of employment, and/or lower rates of price increase, or both.

PART OF THE GAME

Now, in 1965 and 1966, after his overwhelming election victory on a platform of "no wider war," President Johnson began a major escalation of hostilities. But initially, the President was anxious, as part of his "game" with the American people, to obscure the facts of the huge escalation that he was undertaking. It was apparent to anyone who read the newspapers, particularly the *Wall Street Journal*, or any other publication endeavoring to give the businessman or practicing economist an awareness of the sharp increases in war spending as they impinged on the economy. The economics were clear. Expenditures for "national defense" rose from annual rates of \$48 billion in the first quarter of 1965 to \$55 billion in the first quarter of 1966 to \$70 billion in the first quarter of 1967. And orders and the anticipations related to these orders undoubtedly preceded the actual expenditures, so that everyone knew that inflationary pressures were building. Economists recommended an increase in tax rates—and I was among those making such recommendations—as a measure not merely of fiscal responsibility to hold down inflation, but of political honesty, to make clear the economic cost of the war escalation then taking place.

But President Johnson recommended no such tax increase. It did not apparently fit his needs for manipulating American public opinion in favor of himself, his party, and his policies. The needs of the American people, even the value of mobilizing the economy most efficiently for his expanded war, were clearly not the dominant considerations. As the inflationary danger signals grew, the Administration finally put through a back-door kind of tax increase in the fall of 1966 in the form of suspension of the investment tax credit for equipment and restrictions on the use of accelerated depreciation in construction. These were measures, it may be noted, that would not be readily apparent to

the mass of voters, whatever their ultimate effects. And it may be noted, they would also have their real effects on the public, not now but later. For failure to buy new equipment now, or build new plants now, decreases the capacity to produce later. It is by just such curtailment of current investment—whether in plant and equipment or in human beings—that the burden of the war is truly put upon "future generations." Our children and grandchildren are made to pay for our follies of today.

Then, curiously, as the rate of war expenditure escalation slackened in early 1967, and as business apparently anticipated that the big buildup had been largely completed, there were signs of a slackening in the economy. The Administration switched gears again and brought about, with some delay while Congress haggled, the reinstallation of the investment tax credit and accelerated depreciation, so that business lost very little in the way of profits by the maneuvering, although the reputation of the Council of Economic Advisers and some of the Administration's other accomplices was somewhat frayed by the seeming inability to follow a stable course.

THE SURCHARGE PROPOSAL

Finally, just a few months later, in the summer of 1967, the Administration presented its surcharge package, including increases in corporate and personal tax rates and, in particular, a 10 per cent increase (surcharge) in the taxes to be paid by most families or individuals. The President's accompanying message indicated as one justification for the surcharge the simultaneously announced plan to send still additional troops to Vietnam, and also argued that the increased taxes were necessary to reduce the budget deficit and to combat inflation.

The President's tax proposal, as we all now know, did not have clear sailing, and I shall explain shortly why I do not think it should have had. But at the moment I must relate a curious development. A statement of support for the proposed tax increase was circulated throughout the economics profession by Walter Heller (former chairman of the Council of Economic Advisers and still an adviser to the President), Lee Bach of Stanford and Joseph Pechman of Brookings. This statement ultimately received some 275 signatures, including many of the nation's leading economists—but many names were conspicuously missing. The statement carefully avoided explicit endorsement of the war program, which all should have known gave the tax proposal what justification it had, but suggested that the tax increase was necessary on grounds of equity and efficiency and to avoid the sacrifice of desirable domestic programs.

To this statement I did and do take strong exception, both privately and publicly. And, I argue, economists who lent their names and reputations to it have contributed, albeit in most cases certainly unwittingly, to

a debasement of the profession and the confusion of the general public, all in the interests actually of the game being played against the American people by its own political leaders.

On the professional level, it is a bit difficult to see why so many economists jumped when the Administration whips called for support. True, the sponsors were prestigious and the organization and coverage of the petition drive were impressive. And many of us were loath to break ranks with our friends and colleagues in the New Economics who were retaining their close ties in Washington. Further, at a superficial level, this seemed like a textbook case. Government expenditures had risen. We were close to the generally accepted (although perhaps foolishly now) target of four per cent unemployment. Prices had risen significantly and were continuing to rise. We all knew the multiple choice answer on our economic principles examinations: to combat inflation the government should spend less and/or tax more. Since our Leader had decided that the chief component of government spending, the war and national defense, could not be reduced, there was little else realistically to be cut (and in any event we did not want to reduce domestic "Great Society" programs); therefore, the only effective part of the answer was, "tax more!"

But this analysis was at a strangely elementary level. One begins to wonder, a bit sadly, how far the economics profession really has advanced. It is troublesome to think that we may not be able to get through to a generation of college sophomores more than this naive, oversimplified corrective to the budget-balancing mythology by which their fathers still live. But it is sobering to think that so many of their teachers are ready to make policy decisions without any deeper insights into the underlying economic relations, let alone their broader political context.

EXPENDITURES AND TAXES

For the underlying fact is that high government expenditures are not a justification for raising tax rates. Expected increases in government expenditures would offer economic justification for increases in tax rates. Indeed, a central principle of economic theory is that bygone are bygone. Past increases in the rate of government expenditures have wreaked their toll, building into the economy inflationary pressures which are still working themselves out. With the well-known downward rigidity of prices, a new set of deflationary pressures, such as might be introduced by increases in income taxes, might cause reductions in output and employment but could hardly be expected to reduce the price level.

When faced with these arguments, administration economists, privately or publicly, retreated to a new position. Even if government expenditures did not rise further, a new wave of inflation would be brought

on, without a tax increase, by a sharp revival of an abnormally low rate of inventory investment. The argument here was that over the period from the second quarter of 1966 to the third quarter of 1967, accompanying a \$15.5 billion increase in national defense expenditure was a \$12.5 billion decrease—from \$1.4 billion to \$1.5 billion—in the annual rate of inventory accumulation. The sum of these two represented a net stimulus, therefore, of only some \$3 billion. It was then indicated that if inventory investment were to return to a normal rate—it had sunk to as low as \$500 million in the second quarter of 1967—inflationary pressures would develop, even with no further increase in defense expenditures.

This argument falters in two points. First, in terms of the sheer arithmetic, a return to normal rates of inventory investment has nothing to do with the recent war-induced rates of \$14 billion and \$18.5 billion in 1966. During more normal periods, without major escalation of war or national defense expenditures, inventory investment stood steady, as in 1963 and 1964, at just under \$6 billion a year. Arthur Okun, the new chairman of the Council of Economic Advisers, has referred to a "normal prosperity rate of about \$7 billion" for inventory investment. This would actually be a high figure for the immediate future. Most likely estimates were in the neighborhood of \$5 billion a year, and such a rate could well be absorbed in the normal growth of the economy.

Second, inventory investment has been undertaken largely to fill the pipeline of materials and goods in process for the rapid escalation in military durable goods production over the past couple of years. If continued escalation is not taken as a premise, there would appear to be little reason under present economic conditions for any substantial inventory investment at all. Modern economists know well that investment of all kinds relates primarily to growth. One needs more goods, as one needs more plant and equipment, if one is to produce more. Otherwise, one is content with what one has. If national defense expenditures were to remain constant, let alone decline, the major contributor to inventory accumulation would dry up.

Thus we begin to see that a major economic argument for the tax rate increase depended upon a political premise—that there was to be further escalation of the war in Vietnam. This applies as well to the insistence in some quarters that a tax increase was necessary because of balance of payment problems, a substantial matter in itself, which we shall not consider here. But whatever justification the balance of payments offers for a tax increase has related at this time to the huge outpouring of dollars as a consequence of the escalating war. Ironically, many of the economists who signed the statement of support for the Administration's tax proposal are strongly opposed to such escalation. Yet, for a variety of reasons—some of the signers are perhaps just political babes in

the woods, others act out of the reflexes conditioned by long years of teaching elementary economics, and still others can't help but follow the Leader, in the profession or out—they supported specific policy action which could be warranted only by a higher policy they opposed.

The extent of political naïveté may be further gleaned by consideration of the acceptance of a second argument for the tax increase, advanced by some of its advocates within the Administration and outside, that the presumed alternative of limiting or reducing government expenditures for important domestic needs is worse. It is sad that more "liberals" did not see sooner the truth of Walter Lippmann's argument of now a long time ago, that the Great Society and the war in Vietnam were basically and irrevocably incompatible. As Wilbur Mills and his associates have since made abundantly clear, it is politically inconceivable that there will be any major increase in expenditures to meet domestic needs so long as the war continues. Far from facilitating an increase in domestic expenditures, which this Administration has confirmed it does not intend in any event, the very request for a rise in tax rates has tremendously increased the pressure in Congress for a general reduction in nondefense expenditures. Economists might have been expected to realize that those who wish increased government efforts to meet domestic problems in our urban slums and elsewhere can find them only through an end to our military involvement in Vietnam.

There was yet another argument against the proposed tax increase, again related to one's political position regarding the war and escalation of the war. Anticipated increases in gross national product in the current year—apparently taking for granted an upward trend in military spending—ran from seven to eight per cent in money terms, bringing the gross national product to the area of \$850 billion, and some five per cent in real terms. As all new economists know, our progressive tax system has a built-in deflationary bias in a growing economy, since tax receipts rise more than proportionately with money income. But then, given the growth in money income and GNP which is to be expected from the expansion of the economy and the two or three per cent of further price increases that were virtually inevitable over the next year, our current tax structure would cut hard into rising incomes and tend to bring us back to substantial price stability, if military spending did not again soar upward. If there were no further increase in national defense expenditures, one might have expected a somewhat lower GNP, perhaps in the \$830 billion to \$840 billion range, and there would certainly have been no need for a tax increase.

The conclusion is that the existing tax structure could finance current government expenditures with no more price inflation than has become inevitable as the lagged response to the recently past and current increases in military spending. A tax rate increase without a further increase in federal spending would do more to curb em-

ployment and production than it would to curb the rise in prices. A tax rate increase makes economic sense only if one accepts as the political premise a further significant increase in military expenditures.

There were many economists opposed to the war who took another tack. To some of us the tax surcharge proposal threatened to become a sort of economic Gulf-of-Tonkin resolution, offering advance financing of all the additional men and resources the Administration might decide to commit to Vietnam. I was of course under no illusion that failure to pass the tax increase would in itself block escalation of the war, let alone bring it to an end. The President is able to exercise broad powers in the way of military activity at his own discretion. Similarly, he is well able to make military expenditures, with or without tax revenues.

WEIGHING CHOICES

But the President has had to face hard political choices in the conduct and continuation of a war which has been anything but popular. One of the sad facts is that in weighing choices for military expenditures and acts of war it usually becomes clear that the opportunity cost in an economy of less than full employment is far less than the amount of the expenditures involved. But now the situation is somewhat different. Further escalation of the war, if the tax mechanism has not already prepared the diversion of necessary resources, must imply some very palpable costs—more sacrifice of domestic programs, an increased rate of inflation, or new taxes. And any one of these might further strain and split the coalition of political forces on which the President depends. It was just possible that when he called in his advisors, political and economic, to determine the costs and hence the wisdom of some proposed action, whether calling up additional reserves, renewing "clear-and-destroy" operations, sending more troops to South Vietnam, invading Cambodia or North Vietnam, or even adding to our "commitments" in Korea, the President would be influenced in his decision by the fiscal framework within which he was operating. And who is to say that the fiscal constraints creating objections expressed in publications such as the *Wall Street Journal* and *Business Week* did not contribute to the fateful announcement on March 31 (after this paper was written) that new steps for peace were to be initiated and that President Johnson would not be a candidate for reelection?

So a letter opposing the war tax was promoted by William Nordhaus of Yale and sponsored by Wassily Leontief of Harvard; Kenneth Boulding, current president of the American Economic Association, now at the University of Colorado; R. A. Gordon of Berkeley; Stanford's John Gurley, managing editor of the *American Economic Review*; Edwin Kuh of MIT; and myself. The letter had 320 signatures by January 13, when it was released. It argued that "as long as \$2 billion of Ameri-

can resources pour into Vietnam every month, the needy will remain the war's chief victim."

"By this fiscal year's end," the letter said, "more than \$750 for every household will have been dissipated on the conflict. The underprivileged and children in crowded schools are made to wait." "Since Magna Carta," it continued, "whenever persuasion failed to curb the ruler's ill-advised policies, the people's representatives have asserted their sovereignty through the power of the purse." As Boulding has said in a letter to the *New York Times*, "A tax increase now would be the final abdication of Congress. . . . Who is going to call 'halt' . . . if not Congress through the exercise of its fiscal power? A determined stand against the tax increase is perhaps the last constitutional chance to resist the disastrous Presidential leadership to which we have been subjected."

There you have it. The narrowly economic questions aside, should the economics profession lend its support to Administration fiscal measures on the purely technical grounds of fighting inflation? More generally, should economists accept the ends of our political leaders and put their skills to work in the forum of public debate as well as in the inner councils of government, to serve those ends in the most efficient manner possible? Or should economists recognize that political leaders are in a game, with the American people high (if not tops) on their list of adversaries, and with no necessary identity of preference functions (or ends) as between the people and their leaders? Should not the role of economists then be to make clear the costs and opportunities of alternative courses of political action, stating frankly and honestly their own preferences where they are relevant? They would perhaps have most to say on the traditional economic subjects. But the tools of their trade may prove increasingly applicable to all of the complex political issues where ends as well as means must be evaluated and chosen.

Economists should be honest and above board; they should leave no credibility gap. They should not accept a party line that a tax increase must be recommended and then shape their analysis and forecasts of economic conditions to fit that line. If their recommendations are based on certain premises or ends, whether those of the party in power or the party out of power, some segment of the population, some industry or firm, or merely their own, they should make this clear. They should look for problems in society and not merely wait for the questions that are asked them, for indeed, a biased selection of questions is likely, day in and day out, to keep us much further from truth than biased answers. And, if they are to maximize their value as economists, in the final analysis they should, extreme as this may seem, be loyal to no leader or party but rather strive with the skills they have, and should always seek to improve, to be true to themselves, their profession, and the world in which they live.

Arms Control and Disarmament: The Great Evasion

SIDNEY J. SLOMICH

"If we do not change, if we do not look for meaning to ideas and to politics instead of to things and possessions, if we persist in our blindness and particularism, if we do not become alive again to our traditions and better selves, we will burn our wives, our children, our friends, our societies, our great monuments to past sanity, the fields and trees, the innocent animals. Fast death clouds will descend and float about the cinders of what was once a great, wide, beautiful, pulsating, life-intoxicated world."

Sidney J. Slomich recently joined the Stanford Research Institute. He previously served as Manager of Arms Control Studies at the Jet Propulsion Laboratory.

An inordinate proportion of time and effort spent by researchers in arms control and disarmament is devoted to detecting, monitoring, and recording evasion or possible evasion efforts by the USSR, China, and other communist countries. This overconcern is a means of avoiding arms control. So little time and effort are devoted to establishing areas of mutual agreement, mutual interest, and potential new and future areas of cooperative effort with the Soviets and other communist powers that it is possible to conclude that Americans don't believe in it seriously—that in spite of the events of the last 15 years Americans still maintain a bipolar view of the world. As long as this is the attitude toward arms control and disarmament of the main actors and movers, the enterprise from the beginning is doomed.

In any case, we act as if it were, when as a government we avoid political contexts, taking refuge in fantasies of alleged technical solutions. Thinking of foreign policy in terms of technique, whether for "counter-insurgency" or weapons of mass destruction, while pushing into the background and forgetting Western Man's ancient political, legal, and moral traditions and values, is a contemptuous denial of self. How else is it possible to explain the recent decision from Washington, affirm-



ing the intention now to build a "thin" antiballistic missile defense against Communist China? The act does violence to deep political realities and also to the scientific and technical aspects of the challenge of missile interception.

THE MEANING OF THE ABM DECISION

Few find the ABM decision any kind of convincing answer to the security of the United States or the world. When McNamara, in the fall of 1967, announced the decision to build the "thin" antiballistic missile system against China, there was a great deal of ambivalence, logical confusion, and uncertainty in his lengthy speech. The Secretary clearly spelled out the futility of expecting an effective antiballistic missile system to deter the Soviet Union. He stressed the much greater general progress, over the last generation, of offensive systems as against defensive, and the hard fact that large numbers of relatively cheap and simple offensive missiles can saturate any antiballistic system. There is no defense against numbers in this particular game.

Recourse to an ABM system, then, emerges as a negativist fantasy reaction to a political situation which we are unwilling to face because it runs against deep emotional preconceptions and current political commitments, many of them held in doubt by their most authoritative adherents. If this were not the case they could hardly be maintained in such an extreme and illogical fashion. I refer to the scope and nature of the war in Vietnam, American official and *pro forma* fear and hatred of China, American unwillingness to accord recognition to China, and American insistence on conducting foreign policy as if it were completely and irreconcilably hostile to freedom and reason.

It is not only what we have done or what we are

about to do vis-a-vis Chinese missile and atomic warhead capabilities, but also what we neglect. The decision to build an ABM against China is this year's allegedly positive step toward that country, which is going through a convulsion in which the ideological, cultural, Party, economic, agricultural, and even aesthetic manifestations of life are being questioned and drastically reformed. The ABM decision answers nothing and solves nothing; rather, it compounds our difficulties with China and with ourselves. By retreating into this technical-military fantasy, America attempts to reduce China to an atomic warhead and missile factory, and make the same thing of the United States. By looking mainly to military-technical problems and solutions we ignore not only China's political, social, cultural, and international aspirations and hopes, but also healthy American political, social, economic, and cultural interests and values. It is the reaction of manufactured technological and military zombies. Why? Do we fear ourselves so deeply? What inner problems or insecurities are masked by this preoccupation with technology and this rejection of ethics and politics?

There has been a spate of other recent decisions indicating an almost exclusively technical and scientific view of ourselves and the social universe. One example is the plan for an electronic wall across the seventeenth parallel separating North and South Vietnam. This is technocracy going wild. How can we think of splitting a country in two, of putting an end to the flow of ideas, whatever their nature, through hooking up electronic apparatus?

In the same category of excessive preoccupation with technical and scientific approaches is the decision of late September 1967 to redesign the bomber defense system at a cost of several billion dollars. After the

several billions are spent, a relatively inexpensive, indeed "cheap," antibomber defense of a much more modern variety will emerge. Is the Soviet bomber force, in the missile age, so real a menace? Why did this decision have to come at this particular time, along with other multibillion dollar defensive deployment decisions, when the country appears unable or unwilling to finance the reform of domestic injustices and inadequacies which can no longer be borne?

THE SPACE TREATY: PROLIFERATION

Looking at the disarmament accomplishments of the last year and at the matters with which the Arms Control and Disarmament Agency has been primarily occupied, it seems necessary to conclude, however reluctantly, that this Agency gives the illusion of handling problems, while the government avoids getting at the core of what is needed—the control, reduction, and eventual elimination of strategic nuclear weapons. The great accomplishment of 1967 was the conclusion of a treaty on outer space, prohibiting weapons of mass destruction in orbit and the military use of the moon and the planets. Most observers, with the exception of a small number of professional Air Force buffs, have concluded that weapons of mass destruction in space would be of slight marginal utility and would constitute in fact costly diversions. It is difficult to see what this treaty accomplishes beyond reinforcing a dangerous illusion—that in reaching agreement on the treaty on outer space something of great value has been accomplished.

There is some feeling that any arms control and disarmament agreement, no matter how picaresque, is of value because it constitutes "progress." But preoccupation with peripheral efforts which have only slight if any relevance to the broad strategic issues involving arms control is a dangerous business. The space treaty may be a destructive diversion from the most serious arms control problems facing the United States and the world, that of the control, reduction, and eventual elimination of nuclear-tipped strategic missile delivery systems. There is something almost farcical about a treaty abolishing the deployment of mass destruction weapons from space, when there is slight prospect of any country's putting them there, and when no country, under any set of assumptions or drives, needs to, since it's easier to do it on earth. The announcement by Secretary McNamara of early November 1967 that the Soviet Union may be developing a fractional orbiting bomb does not change this at all. The Soviets are not above making serious mistakes and poor allocation of their resources and efforts.

We must reluctantly reach similar conclusions with regard to national and international preoccupation with a general treaty controlling the proliferation of atomic weapons. It is difficult to become exercised over vaguely threatening possibilities of nuclear-tipped ballistic mis-

sile delivery systems in countries like Argentina, Brazil, India, Indonesia, Malaysia, Costa Rica, and the Congo.

Only a few countries have the motivation to build and to deploy nuclear-tipped ballistic missile delivery systems. They are states with unique traditions or positions in world affairs, or with long-term conflicts involving their very existence. China and France have developed nuclear weapons apace, and one cannot exclude the possibility that within the foreseeable future such weapons might appear in the Middle East. India has been muttering about its need for atomic weapons, in view of Chinese threats, but India's security can still be settled in terms of broad political and diplomatic arrangements.

We must not downgrade the danger that new peaceful atomic breeder reactors will eventually produce, as a by-product, massive quantities of fuel suitable for the manufacture of atomic bombs. What is maintained here is that the proliferation is very far from being the main problem and that to a significant extent, this year, next year, and for a number of years beyond, it constitutes a diversion from the main menace—uncontrolled stockpiles of strategic nuclear weapons. We need to survive first so that proliferation can become a problem.

TECHNIQUE IN VIETNAM

Leaving aside for a moment the strategic nuclear picture, let us consider the Vietnam conflict in terms of this same syndrome of looking for political solutions through the application of technical capabilities. One recent "solution" has been defoliants. We have turned to crop destroyers. The interest in helicopters goes back to the early sixties. Now there is the electronic barrier and bombing. What all of these have in common is technique, and not preoccupation with the politics and historical factors and traditions which started the conflict and in terms of which it will end. In early 1968 the chief U.S. means of bludgeoning the North Vietnamese to the conference table (and the Vietcong, if we were indeed interested in talking to them) was bombing and other technical military "solutions."

On the surface there seems to have been a great deal of effort to come to grips with broad social, economic, and political problems in Vietnam. There were the strategic hamlet programs, the recent attempts to build a constitutional framework in Vietnam, and "pacification," along with stop and go attempts at rural reforms. But these programs have largely been as technocratic as the military, for as a whole they constitute only a holding operation to reinforce a (hopefully) stable government of some kind as a backdrop against which the main effort goes on—bringing about a solution to the problem of Vietnam through bombing and other technical-military capabilities. If this were not the case, then there would be going on in Vietnam—supported by a great deal of effort in the Pentagon, the Department of State, and other agencies, the universities, and the

American public—open, searching, questioning activity centered around how to help the Vietnamese build a free society in which there could be more than one feasible pattern of political organization, including some not necessarily palatable to the United States or to the present Administration. A single political approach to the organization of political life in Vietnam is recourse to technocracy.

There are other areas of the world in which similar U.S. patterns of trying to solve potential or actual "counterinsurgency" problems have been viewed in essentially technical-military terms. In the early fifties the Eisenhower administration began to proliferate conventional arms to Latin America, in response to the possibility of communist threats from abroad or internally. In some countries, where the military was not particularly rich in equipment, and which in any case had been moving away from militarism, the clock was turned back, as a relatively few tanks, a cruiser or two, or a few fighter aircraft modified radically the internal balance of power. This was evident in Peru, following the elections of 1962, when the military, led by an officer decorated the previous November by Secretary McNamara for devotion to constitutional democratic principles, led five Sherman tanks against the presidential palace and took over the government. In the Peruvian case President Kennedy for more than a brief time withdrew the American ambassador. He did not return until promises of free elections were secured. These promises were observed.

THE WASHINGTON STRUCTURE

Is there any possibility of the situation changing within the foreseeable future, given the organizational structure in Washington? It almost appears to have been designed to dilute and avoid serious arms control and disarmament decisions. There is the Arms Control and Disarmament Agency (ACDA) housed in the Department of State and responsible to the President. Certainly the director and some of the staff have had a long and distinguished experience in the subject, going back to the middle fifties and beyond. This small agency is now more than six years old, and there is an old Washington saw that an agency goes to seed after five years.

Over the last two years there have been a significant number of resignations at the key upper working level of ACDA, and one would be hard put to find more than a fraction of the original, creative, lively, and rather iconoclastic people present in its early years. Most of those in positions of responsibility are good, well-motivated, experienced Washingtonians—not usually the types to take unusual risks or push beyond what they think the system might take.

This is not an organization against which to level bars; it remains the principal official organization through which arms control proposals are formulated

and negotiated, and it is in a real sense the keeper of the country's conscience. If the government and the country had wanted to see ACDA stronger in 1967 than it was in 1963, that strengthening would have occurred. Further, in the last couple of years ACDA has to some extent become a butt for other organizations and for the Congress, just as the United States Information Agency or the Voice of America were during the Joseph McCarthy period. ACDA is not powerful. It is not only small in numbers, but very small in funds. Its annual spending for all purposes runs around \$10 million. It is subject to being kicked by others, especially those to which dealing with problems of treaty evasion, inspection, and other matters considered crucial and esoteric, tie in with still fashionable and long-standing forms of paranoia. Some groups still consider the ACDA un-American—one large veterans' group for several years has been sending formal depositions to Congress demanding its abolition. It is not a permanent agency, and one wonders whether it ought to be. The case could be made that keeping it independent has been a means of directing responsibility for arms control and disarmament away from the larger agencies, notably the Pentagon and the Department of State. Perhaps these large, rich, and more visible bureaucracies ought to be clearly saddled with the responsibility for formulating national policy relating to arms control and disarmament.

What are the relationships between the ACDA and other governmental agencies? There is a Committee of principals, an interagency group composed of the heads (or their deputies) of the Pentagon, Nasa, the Department of State, and others, and, of course, ACDA. This is the National Security Council, so to speak, for arms control and disarmament. The interests of the Department of Defense are hardly identical with those of the ACDA, and others, especially those with a sense of mystery and difficulty in dealing with the Soviet Union and other communist states, may also see things differently from ACDA.

The office of the Secretary of Defense has, in its International Security Affairs organization, an able senior advisor responsible for arms control and disarmament questions. His research budget is tiny, and his staff limited to a few military officers, who are there on a rotating basis, and possibly a civilian or two. If a government were purposely to organize a staff with built-in difficulties for grappling with disarmament issues on a sustained basis, an organization designed to lose periodically whatever expertise it acquired, this is the setup that would ensue.

The funds for critical study of proposals on arms control and disarmament within the Pentagon may be as great or greater than those which the ACDA controls. Not only must the particular budgets of the Services and the components of the Department of Defense be considered, but also the fact that whatever the

size of their budgets for formal arms control research, they can draw informally upon other considerable resources and the think tanks.

We must not assume a monopoly of virtue or insight in ACDA, much less a significant predilection for substantive as against sterile technical approaches. If we look through the annual proceedings before Congress, when ACDA comes up for its pitiful budget and lists its research projects, we will see there are few dealing with politics and general international relations and internal Soviet and Communist Chinese policies and their relationships with arms control, while there are many on monitoring, censors, and so on. ACDA has a very small and able corps of individuals dealing with the broad political questions and negotiations, but most of the research allegedly probing into basic matters seems overcommitted to trying to find technical solutions to political problems.

IMPLICATIONS AND DIRECTIONS

Implications are clear: if the problem is fragmented among a number of agencies, if too much of the original drive for accomplishment is diluted or gone, if too many restless people are also gone, if agencies which can hardly be expected to have strong drives toward arms control and disarmament are peer organizations on an interagency committee, if the temper of the administration (if not the country) is for technical rather than political and social approaches, then the public interest is not being served.

Agencies of government concerned with military capabilities, research and development, and related affairs tend to claim monopolies of wisdom. The existence of classified information compounds the public's difficulty in getting at the issues. But concerned people need not bend the knee to possessors of classified information. There is nothing in it which can repeal the laws of physics or engineering, or whatever we know of politics and society. Above all, classified information tells us nothing about ethics or constitutional morality.

There is need for more concern with broad arms control issues in the universities, in research institutes which are not governmentally supported, in business firms which do not have heavy dependence on government contracts, and other research and political organizations. These affairs must not be left to the entrenched and rigid habits of thought of those currently enjoying a near monopoly in their disposition.

Research, however, is no substitute for political action. What we need is not agitation; not vague, inchoate pleas for peace, along with self-righteous declarations against war; not fantasy-ridden calls to abolish the nation state. Rather we need informed, balanced, and active interest and action rooted in ethics, psychology, and appreciation of the politics and technology of modern weapons of mass destruction in the context of the politics of international relations. This will re-

quire knowledge and understanding of basic internal economic and political choices, of the long-range weapons potential of a number of scientific and technological fields, and, above all, a high degree of national and rational self-awareness and self-knowledge, no less than knowledge and understanding of the opposition.

It will not come easily. There can be no decisions, no political commitments, no international commitments until there is built within the country a national constituency, an informed consciousness actively expressing broad understanding and acceptance of specific agreements. It was achieved in the case of the 1963 Test-Ban Treaty. Without such a broad constituency, arms control and disarmament decisions will continue to be made on a narrow bureaucratic and technocratic basis. This will mean, almost certainly, more technical offensive and defensive weapons systems of great cost and dubious value; ignoring or pushing into the background great affairs of state and society; and the growing possibility of an accident, or of the emergence of sufficient irrationality on one side or another to result in a mass exchange of nuclear bombs.

Yet no villainous bureaucracy has consciously frustrated the emergence of a national constituency on arms control or is stifling a national passion for arms control and disarmament. If there has been, over the last generation, a general feeling on the subject among the educated, the influential, the scientifically, technically, and politically competent, it has been that we need for our national security more and more powerful and sophisticated weapons of mass destruction and little else.

Giambattista Vico, the great eighteenth-century philosopher of history, maintained that political history was cyclical and that toward the end of the cycle there appeared a mass democratic society foredoomed to collapse through inevitable decay of civic virtue and responsibility. This collapse occurred, in his view, because Providence worked to bring it about so that men could have another chance to play out the drama of salvation and the search for civilization and reason.

The existence of atomic weapons means that there may not be another chance, so let us not accept this particular, philosophic counsel of despair, any more than we ought to accept the technocratic view of the inevitable proliferation of offensive and defensive missile systems.

What, then, of the prospects? If we do not change, if we do not look for meaning to ideas and to politics instead of to things and possessions, if we persist in our blindness and particularism, if we do not become alive again to our traditions and better selves, we will burn our wives, our children, our friends, our societies, our great monuments to past sanity, the fields and trees, the innocent animals. Foul death clouds will descend and float about the cinders of what was once a great, wide, beautiful, pulsating, life-intoxicated world.

Recent Eddies in Brain Currents

SAMUEL SUTTON

"Unlike . . . other codes this language of the brain only appears to be arbitrary. . . . Yet it is a savoring thought that even if what we are decoding is not arbitrary—and is indeed God's handwriting—it is unlikely that it will provide an intuitively satisfying understanding of the workings of the mind. Even if we were finally to reach the point where we could record all the relevant brain activity at the moment when an artist creates a great masterpiece, I doubt that we would be able to . . . say: 'Ah yes, now I understand; now I know what the act of artistic creation is!'"

Samuel Sutton is principal research scientist at Biometrics Research, New York State Department of Health, and an associate professor of medical psychology at Columbia University. The following article is based on a Fard Foundation Basic Science Lecture delivered at Bennington College, May 26, 1966. The research described was supported in part under a United States Public Health Service Grant.

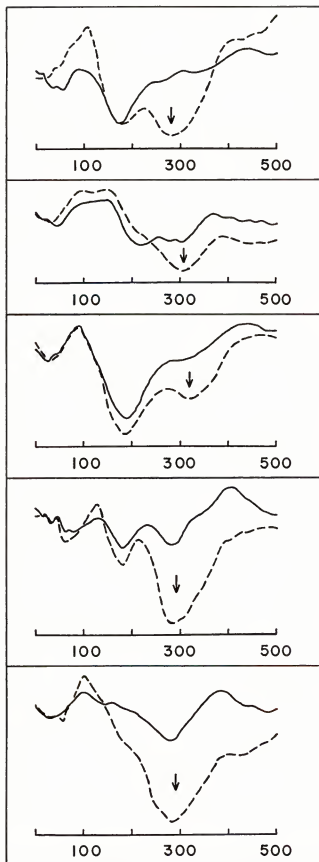
In *The Royal Hunt of the Sun*, a play about the Spanish conquest of Peru, there is a scene in which the Inca ruler is shown as first becoming aware of the idea of writing. He is amazed that he can whisper a word, such as "God," to one person, that this person can make a non-pictorial squiggle, and a second person—not within earshot—can look at this "meaningless" squiggle and speak the correct word aloud. If we permit ourselves to empathize with the Inca king, we can see that there are two aspects to his amazement: one, that a thought can be communicated in a completely novel way; and two, that the squiggle has none of the properties of the concept it represents. Upon reflection, one might assume that the king should not have been so surprised at the arbitrary relation between the written symbol and the concept, since an equally arbitrary relation exists between the spoken word and its referent. However, I have recently come to realize that the sense of strangeness and awe that follows discovery of a new symbolic code is not diminished by even a sophisticated appreciation of the arbitrary nature of all codes.

In the last several years, I have been recording the electrical activity of the brain from the surface of the scalp in awake human subjects. Recording the brain's electrical activity is not new; the electroencephalogram was discovered nearly half a century ago. This technique has been enormously important for the detection of brain damage and particularly for the localization of epileptic foci. The electroencephalogram also reflects differences in the activity of the brain between such

general states of the organism as sleeping and waking. However, only in the last decade have we begun to be able to relate the electrical activity of the brain to more specific psychological states involved in the process of thinking. This new type of information about brain activity has been made possible by advances in computer technology.

Now, these recordings of the brain's electrical activity are just squiggly lines, no more comprehensible than writing to the untrained eye. They record the rate at which a particular point on the scalp becomes more electrically positive or more electrically negative than another point on the body. When most of the data came from work with animals it was somehow acceptable that one kind of "squiggle" was recorded in response to a flash of light, and a different kind of "squiggle" was recorded in response to a sound. When the responses are obtained from human subjects, however, one's reaction to the squiggles is another matter, as I discovered in the course of my own work. My empathy with the feelings of the Inca king arises from the awe I felt when I would hand over one of these squiggly lines to an assistant and find that she was able to say: "Oh, that's Jo Roberts when she makes a mistake," or "that's Dorothy Jacobs when she sees a flash of light and she knew ahead of time that it would be a flash of light." These squiggles are different from subject to subject: they seem to carry a personal signature. At present, however, we cannot interpret these individual differences. But what is startling at our present state of knowledge is that we can begin to decode the language of the brain—for example, what any brain's response is to being wrong or right. And, of course, it seems quite an arbitrary language. When we first examined the relationship between these squiggles and the psychological variables of our experiments, they had the same lack of obviousness that the ABCs have for a child. The lines could have gone up where they go down, and down where they go up, and we would have been just as happy. Of course, there is a difference: unlike these other codes this language of the brain only appears to be arbitrary. In fact, as we find that the shape of the squiggles is related to the activity of different populations of neurons, the code will become less arbitrary. Yet it is a sobering thought that even if what we are decoding is not arbitrary—and is indeed God's handwriting—it is unlikely that it will provide an intuitively satisfying understanding of the workings of the mind. Even if we were finally to reach the point where we could record all the relevant brain activity at the moment when an artist creates a great

RESPONSES TO CERTAIN AND UNCERTAIN CONDITIONS



Solid line indicates response in certain condition; broken line, response in uncertain condition. Time is shown in milliseconds.

masterpiece, I doubt that we would be able to look at the mass of wavy lines and say: "Ah yes, now I understand; now I know what the act of artistic creation is!"

CLICKS AND FLASHES

The charts on these two pages show the differences in electrical potential for several subjects—they decode the language of the brain.

The graphs shown in column 1 ("Response in Certain and Uncertain Conditions") illustrate some of these wavy lines recorded for five subjects. They represent the differences between a point on the scalp and a convenient standards or reference point—in our experiments, the subject's earlobe. The tracings represent a process going on over a period of time, so at the bottom of the graphs time is shown in milliseconds. By arbitrary convention, when the lines are going up the region of the brain below the electrode is going electrically negative with respect to the earlobe reference; down is positive. Each pair of superimposed curves is obtained from a different subject. These tracings were obtained from an experiment in which subjects were asked to guess whether the next event in a series of stimuli would be a click or a light flash. The dashed lines show the electrical activity obtained in response to clicks which followed and confirmed, or disconfirmed, the guess. The solid curve is the electrical activity to identical clicks; but instead of guessing, the subject was told in advance that a click (and not a light flash) would be presented. Note that despite the fact that each pair of tracings is different from the others—the personal signature I spoke of—there is a common difference between the states of certainty and uncertainty. When the subjects are uncertain there is a positive-going process which reaches its maximum in the vicinity of 300 milliseconds (see arrows). This process is virtually absent when the subject is certain. These curves, to use the formal term, average evoked potentials, although quite different from subject to subject, are routinely reproducible for each subject. It is interesting in this context of individual differences to note that it has been reported that identical twins have very similar average evoked potentials.

Why these curves are referred to as average evoked potentials requires some explanation. First it is necessary to point out that when one records the brain's activity from the surface of the scalp instead of directly from the brain itself, the signal obtained is quite small. In order to detect responses which are only millionths of a volt in amplitude, we must amplify the potentials at the electrodes about 30,000 times. In addition, the responses to our stimuli are obscured by many types of electrical "noise" consisting of ongoing activity of the brain unrelated to our stimuli, as well as activity from muscles, skin, and artifacts which arise from the recording instruments. In the graph on page 25 ("Individual Responses to Clicks") all but the bottom trac-

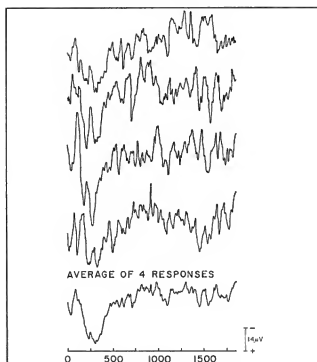
ings are single responses to identical clicks. Yet note how different they are! Each of these tracings may be viewed as some mixture of the response-to-click with the noise which happens to be simultaneously present. For the four single responses shown in the graph, suppose we obtain an average at each fractional point in time. The result is the average curve which is shown at the bottom. This average curve is assumed to give a truer representation of the response-to-click than any single response, since the random noise has been cancelled out by the averaging, while the activity in response-to-click which is not random will not cancel out. Normally, we do this averaging, not over four, but over a hundred or more click presentations.

FURTHER FINDINGS

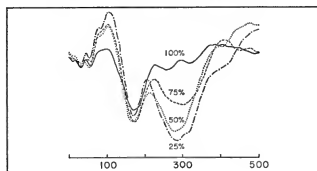
I have indicated that there is a positive-going process which is present when an event occurs about which the subject is uncertain, but which is absent when the identical event occurs in the context of certainty. But certainty and uncertainty are not absolute states. One may be moderately certain or moderately uncertain. The obvious question is: Can the amplitude of this positive process be related to the degree of uncertainty? For example, suppose in a series of trials we present in random order either clicks or light flashes, but clicks are presented three times as often as light flashes. The subject is still uncertain whether clicks or light flashes will occur, but less uncertain than he would be if clicks and light flashes were presented with equal frequency. If he were asked to guess before each presentation whether the next event would be a click or a light flash, the subject would appreciate the inequality and would more often guess clicks, and would more often be right. By varying the proportion of clicks to light flashes one may obtain any desired degree of uncertainty with respect to the occurrence of clicks. In one experiment we had four different conditions using different relative probabilities, or degrees of uncertainty, of clicks to light flashes, one in which clicks were presented in 100 per cent of the trials (complete certainty), one in which clicks were presented in 75 per cent, one in which clicks were presented in 50 per cent, and one in which clicks were presented in 25 per cent. The graph in column 2 ("Effect of Relative Probability") shows the responses to clicks at these four degrees of uncertainty. It can be seen that the lower the probability of click occurrence, the larger the amplitude of the positive (downward-going) process in the vicinity of 300 milliseconds.

We have shown, therefore, that the amplitude of the positive process is related to the degree of uncertainty. We can relate the positive process to the issue of uncertainty in quite another way. This has to do with the time at which uncertainty is resolved. Consider the following experiment: The subject may be presented in

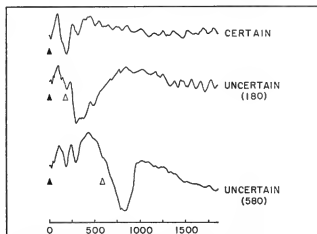
INDIVIDUAL RESPONSES TO CLICKS



EFFECT OF RELATIVE PROBABILITY



WHEN THE CLOCK DOESN'T STRIKE



Time in the above charts is shown in milliseconds.

any given trial with either a click or a double click. The double clicks consist of two identical clicks separated by a fraction of a second. In the certain condition he is always told whether the next presentation will be single or double. In the uncertain condition he is not told and he is asked to guess.

In the graph on page 25 ("When the Clock Doesn't Strike") we have as the first tracing the average response to single clicks when the subject is certain. He had been told that single clicks would occur and single clicks were presented. Both the second and third tracings were also obtained in response to single clicks but these were obtained when the subject was uncertain. However, the second tracing was obtained in an experiment in which the alternative double click contained a 180 millisecond interval between the members of the click pair, whereas the third tracing was obtained in an experiment in which the alternative double click contained a 580 millisecond interval between the members of the click pair. It can be seen that while the response to single clicks for both uncertainty conditions contains a large amplitude positive process, it occurs later in time in the lower tracing.

The latency of the positive process is evidently linked to the point in time at which uncertainty is resolved. The reasoning which leads to this interpretation is as follows: Since both these tracings are responses to single clicks, in these trials the subject can only discover that the stimulus is not double by the absence of a second click. For the second tracing, absence of the second click can be detected at 180 milliseconds since this was the interval in the double click. For the third tracing, absence of the second click cannot be detected until 580 milliseconds since, in this experiment, this was the interval in the double click. These points in time at which the second click might have occurred are indicated by open triangles. It can be seen that the large down-going process in these two tracings remains in constant relationship to the point in time at which a second click might have occurred but did not occur. It is later in the third tracing than in the second tracing because in this condition the discovery of the absence of the second click could not occur until later—after 580 milliseconds have elapsed. This physiological registration of the absence of an event is reminiscent of a line in an old Danny Kaye film in which a woman interrupts herself to say "I distinctly heard that clock not strike."

We have therefore demonstrated that the positive process is related not only to the degree of uncertainty but to the point in time at which uncertainty is resolved. In other experiments, we have shown that the positive process is altered by many experimental conditions which alter the significance of the stimulus to the subject. For example, the amplitude of this component is smallest if the subject is instructed to observe sequences of sounds and lights passively, somewhat larger

when we instruct him to count all stimuli, still larger when we ask him to count only sounds or only lights, and largest when the instruction is to guess whether the stimuli will be sounds or lights.

Given this formulation in terms of a concept of significance, the results of our next experiments were particularly interesting. Since we were instructing our subjects to guess, we were able to obtain two average evoked potentials to identical stimuli: one when the subject's guess was right and one when the subject's guess was wrong. We found that for most subjects the amplitude of the late positive component was larger for wrong guesses than for right guesses.

What did this mean? Given our earlier interpretation—that the amplitude of this positive component was correlated with the degree of significance of the event—then this experiment seems to reveal that it is more significant to be wrong than to be right. Our first amusing speculation was that we had tracked down the brain correlates of original sin. Still another speculation was stimulated by my many years of discussions with the philosopher Kenneth Burke. He had consistently emphasized the key role of the concept of the negative for symbolic activity. Finally, a more trivial possibility was that since most of our subjects were university students, perhaps years of taking examinations had taught them that being wrong was more significant than being right. Either because of the present state of the science, or because my mind is too prosaic to deal with these larger issues, none of these speculations led to any fruitful experiments.

We turned our laboratory into a gambling casino and had the subject win or lose money as a function of guessing right or wrong. We observed that the amplitude was larger when we made the subject gamble for dimes than for pennies. By arbitrarily manipulating the stakes so that if the subject won, he won a dime, but if he lost, it cost him only two cents, we were able to get larger amplitudes for right guesses than for wrong.

BRAIN LANGUAGE

Having presented some specific data, I shall now reconsider the assertion with which I began: that we are learning the language of the brain. This is a complex and rather subtle issue and I would like to introduce it with an analogy. Suppose a Martian scientist was attempting to study life on earth and he had the choice of either building a telescope capable only of high magnification or of building a telescope capable of only low magnification. Under low magnification, he could encompass an entire area like a college campus. He could observe the general increase and decrease of activity correlated with the diurnal cycle and with the seasons; and although he might be able to pick up any general hubbub of activity, he might not learn anything particularly unique to college life. Now, let us surmise that under high magnification he could pick up

the activity of a single individual. Depending on whether he focused upon a gardener, or a student, or a professor, or a dean, he might become aware of different aspects of campus life; and if he were fortunate in his choice, he might learn something quite unique; however, if he were not fortunate, he might not learn anything useful. Or, to take another analogy, suppose the telescope was pointed at a collegiate football game. I think that it can be seen that the Martian scientist would do better to take in the whole than to follow the actions of any single individual.

We know that the brain is composed of billions of neurons involved in terribly complex circuitry. When we record evoked potentials from the scalp we are recording something equivalent to what we get with our low magnification telescope. On the other hand, we know from work with animals that each of the billions of neurons in the brain is capable of very finely textured activity—there are single neurons in the frog's brain which respond only when an object about the size of a bug is moved in front of the eye but do not respond to the movement of other objects. There are single neurons which respond when an object is moved in front of the eye in one direction, but do not respond when the same object is moved in another direction. Some neurons are excited (increase their activity) by a given stimulus and other neurons are inhibited (reduce their activity) by the same stimulus. When we record from the scalp with our gross electrodes, we wash out all this fine texture and consider only gross averages over billions of neurons.

If the language of the brain is contained primarily in the activities of single cells and their interconnections, then the technique I have been describing will rapidly run dry. It is simply revealing fortuitous correlations, just as in the Martian's low magnification telescope the fact that all the dormitory lights go on in the evening and go off later, would register something without telling him anything uniquely important about college life. On the other hand, if the language of the brain is based on the joint activities of large aggregates of cells, with the activity of any single one being of minor importance—like the effect of the rise and fall of any single stock used in the Dow-Jones averages—then we can expect to forge ahead very rapidly with the available techniques. With our present techniques we are either learning something about the way in which the brain actually codes information or obtaining only secondary reflections of brain activity coded at another level.

This basic question cannot be answered at the present time; in fact, posing the alternatives in this way may have only heuristic value. We have, for the time being, placed our money on the low magnification approach. So far, it continues to pay off. By this I mean we can touch on different areas of psychological function and obtain brain correlates. Recently, we have obtained different evoked potentials as a function of whether the subject is visually presented with squares or circles.

There is, however, little difference between large squares and small squares. These findings would suggest that we are getting at the evoked potential correlates of visual form. In another laboratory it has been shown that different colors give reliable different evoked potentials. The more we succeed in obtaining differences in evoked potentials which are related to differences in psychological events, the more confident we feel that we are getting at the language of the brain.

In our own laboratory, we have taken average evoked potential waveforms such as I have presented and have had some success in fractionating them into different components. We can identify components which are related to the nature of the sensory stimulus (for example whether it is a sound or a light); components related to the nature of the guess; and finally components related to the emotional reaction to being right or wrong. With respect to the latter, we can use quite different stimuli in the experiment, yet the components associated with the emotional reaction to being wrong or right remain essentially unaltered.

THE POTENTIAL

To the student of the brain, these developments carry great promise. Learning something of the brain's activities in relation to psychological processes is of itself a fascinating endeavor. But is there anything here which is of more general significance? After all, we all knew before we began recording evoked potentials that some events were more significant than others, and that squares were different from circles. Wider possibilities begin to emerge, however, when investigators report differences in evoked potentials between hospitalized schizophrenic patients and normal patients. It has also been reported that evoked potentials correlates are found with the degree of improvement of these patients with therapy. Findings such as these have potential practical utility as well as the possibility of telling us something of theoretical significance with respect to the nature of mental illness.

Aside from such problems as mental illness, there is the possibility that these techniques may be useful for the study of psychological function. A recent "negative" finding reported by one laboratory is interesting in this context. As is generally known, an individual can be hypnotized so that he will be insensitive to certain stimuli; or one may suggest and have a subject accept that a bright light is dim and a dim light is bright. When evoked potential recordings were obtained during hypnosis, they followed the real stimulus and not the misleading suggestion made by the experimenter. The evoked potential to a more intense light has a larger amplitude, even if the hypnotized subject "thinks" he is seeing a dim light. This may mean that we have not tapped the relevant brain correlates of the hypnotic state. On the other hand, perhaps the evoked potential work is telling us that the hypnotic state is quite different from what it appears to be at first blush.



ALEXANDER ALLAND, JR.

War and Disease: An Anthropological Perspective

"... The kind of conventional warfare that has been raging for the past three years is, intentionally or not, a kind of covert biological warfare. The continued imposition of stress conditions upon an area with a delicate health balance will, in a variety of direct and indirect ways . . . subject the inhabitants to the same type, if not the same degree, of pathological devastation as that brought about by overt action involving biological and chemical agents."

Alexander Alland, Jr., is assistant professor of anthropology, Columbia University. The following is reprinted with permission from the December 1967 issue of *Natural History*.

Warfare and disease are closely linked. The historical record is crowded with associations between war and the outbreak of major epidemics. The reasons behind such associations, both biological and behavioral, are far more complicated, however, than is generally assumed.

While public health as a matter of policy is new to most countries, all human groups, even the most primitive, have made considerable behavioral and genetic adjustments to the common diseases in their environment. The genetic adjustments result from the selective effect of disease on populations, and more than one author has suggested that disease has been a powerful agent in the course of human evolution. Behavioral adjustments to disease are of two sorts: preventive and therapeutic. Effective therapeutic procedures require conscious effort, including both the development of accurate diagnostic techniques and the discovery of curative agents. In many, if not most, primitive and peasant societies the diagnostic process is concerned primarily with etiology. Disease may be linked to transgression of the moral code, to social enmity, or to supernatural power, with consequent treatment of these "causes" rather than of the disease-producing organism. More often than not primitive medical opinion fails to produce a consistent therapeutic orientation.

It is true that many non-Western societies do have effective remedies for the treatment of minor ills, and of course, certain primitive or peasant remedies contain powerful agents, useful in ameliorating such serious conditions as malaria (quinine) and heart disease (digitalis). In general, however, "native" drugs have little effect upon specific disease agents.

I submit, therefore, that of all the adjustments a prescientific population can make to disease, the most effective are genetic in origin or involve behavior that can be classified as preventive medicine. The term "preventive medicine" is used here to include any medically or non medically directed behavior that, in fact, has a positive or disease-reducing effect on the epidemiological patterns of a population: for example, bathing.

Many behaviors among prescientific peoples are effective in combating or preventing disease. These range from cooking procedures, which lower the risk of intestinal parasite infestation, to customs that require individuals to defecate away from habitation sites and trails. Quarantine for at least some infectious diseases and avoidance of contaminated water are also widely reported.

Epidemics occur when the bacillus is passed on to highly susceptible domestic rodents which rapidly die out. When this occurs their fleas are forced to search out new hosts, the most common of which is man. While there has been some dispute over whether the European plague was a cause or an effect of social disorganization, most authors today feel that the great epidemics of the thirteenth to sixteenth centuries were the outcome of Malthusian pressures and followed in the footsteps of social, political, and economic troubles. According to R.S. Roberts, a medical historian:

Plague was endemic for a long time . . . as we shall see, [it] was a disease which could become epidemic only at a particular stage of socio-economic development, which demographers call "oriental."

At the beginning of the outbreak, England was much more densely populated than is usually thought. From 1086 (Doomsday Book) the population seems to have trebled from 1 to 3.5 million. In the absence of technical advance or import of foodstuffs, this greatly increased population could only be fed by a commensurate increase in the area of cultivated land. The inclosure of waste land and the settlement of new villages in the thirteenth century shows that this indeed was done on a large scale.

It was the social and economic crisis of the 1340s, however, which seems to have created the necessary conditions—rats brought into closer contact with man in the search for food after bad harvests, movements of populations after famines and epidemics, overcrowding in certain areas, and a general decline in health and resistance to disease.

Judging from Roberts and other sources, the maintenance of plague in England, and perhaps other parts of Europe as well, for over 300 years was facilitated by the destruction of woodland, which was converted for agricultural purposes. Such incursions into the native habitat of wild rodents, the endemic reservoir of plague, brought these animals into closer contact with the domestic rat population. As noted above, domestic rats are highly sensitive to plague and, as they die out, the human populations among whom they live rapidly pick up the infection. The epidemic cycle was probably completed when the natural habitat for wild rodents was reduced to the point where it could no longer support the reservoir population. In this case it would appear that the partial destruction of an ecological zone increased disease risk although further incursions into the same zone eventually led to an extinction of the endemic focus of the disease.

PLAGUE: SOUTHEAST ASIA

The parallel between this situation and Southeast Asia is striking. Plague and other acute infectious diseases exist in endemic foci throughout the area. The flood plains of the Mekong Delta support one of the

This is not to say that all behaviors are positive in their effect. Indeed, many customs increase medical risk for particular groups. And certain cultural practices that are relatively safe under one set of conditions increase risk if the conditions are changed. Small populations are less likely than large populations to be subject to epidemics. It is also true that the sudden migration or displacement of populations from one ecological zone to another may expose them to diseases against which they have no genetic protection.

In general, health patterns depend upon ecological relationships between man, disease organisms, and vectors, as well as upon their reservoir populations. These must be considered as part of the behavioral framework rather than as part of the "natural setting" of a population. Such relationships are always tentative, considering what man can do to his environment, and there is much evidence to show that the natural setting is rarely natural.

It is unfortunate that no studies exist that weigh positive and negative behaviors against each other. There is evidence, however, that disturbances of normal (meaning long-term) ecological or behavioral patterns usually lead to increased disease rates and lowered viability. This certainly suggests that the net effect of socially established genetic and behavioral adjustments undergone by prescientific societies is, in fact, positive.

In sum, genetic and behavioral adjustments to disease are part of a general process of adaptation to environment. Public health, particularly in prescientific populations, depends upon such accommodations. I am convinced that the view of public health outlined above is necessary if we are to understand the dynamics of adjustment to disease and the effects of war on such adjustments.

PASTEURELLA PESTIS

An example from European history has implications for the present situation in Southeast Asia. I have chosen plague for illustrative purposes because its occurrence is well documented; its etiology is complex but well understood; and it is a severe, highly contagious disease sensitive to ecological variation.

The agent of plague is the bacillus *Pasteurella pestis*. It is a disease with a short incubation period, florid symptoms, and a high mortality rate. Plague has a complicated etiology involving wild and domestic rodents and a flea vector, with the normal endemic focus located in wild rodent populations (sylvatic plague).

highest population densities in the world. In addition, the agricultural complex constitutes an ecological niche quite apart from the wild mountain zone. Until recently, Vietnamese from the plains were afraid to venture into the mountains, often citing exotic diseases as the cause of their anxiety.

Hygiene and public health measures are primitive, but under normal conditions, severe epidemic outbreaks are limited in their distribution and scope. While the population is certainly not the healthiest in the world, a delicate balance exists that has allowed people to survive and multiply in great numbers. This is due in part to the incredibly productive nature of the delta area.

Although epidemics of the plague have not occurred for some time, the number of cases in Vietnam has mounted steadily and sharply over the past three years. On August 20, 1966, the *New York Times* noted that at that time the plague had infected 22 of 29 provinces stretching north of Saigon, but that in 1961, plague was reported in only one of those provinces. The same article presents the following statistics on plague incidence in South Vietnam: 1954-62, less than 40 cases per year; 1963, 119 cases; 1964, 290 cases; and 1965, 4,453 cases. The Reverend Do Van Quy, chief of the Pasteur Institute's plague laboratory, predicted: "This problem will not be solved until the war is ended." Dr. Joe Stockard, chief of the U.S. aid mission's preventive medicine section, said: "Now that the disease has become so widespread, a high incidence may be expected to continue for an indefinite period."

Extremely crowded conditions in the cities with overstrained and primitive sanitary facilities are no doubt a major factor in the statistical increase. American and South Vietnamese authorities are aware of the problem, at least insofar as it relates to cities, and a program of rat elimination is under way. The dangers inherent in such a program are minimized by the existence of a relatively effective plague vaccine and effective antibiotic therapy. Nonetheless, a case of the plague in an American serviceman was considered important enough to be included in the *AMA News* for December 1966.

Rat elimination is a rather quixotic pursuit in Southeast Asia. The ability of rats to survive even in the urban areas of the United States attests to their hardness. Rats flourish in the primitive sanitary conditions of the slums and bidonvilles of Vietnam. It would seem, therefore, that an effective antiplague program should be more concerned with interrupting the routes of infection from the endemic focuses to the domestic rodent population. What has happened may be the reverse! Defoliation and extensive napalm attacks may have produced disturbances in plague reservoir areas sufficient to increase the contact between wild and domestic rodents. While I must stress that this is mere conjecture, such an explanation fits well with current theories of plague epidemiology. In addition, the fact that plague is virtually absent in the Mekong Delta

region for "reasons" again according to the *New York Times*, "that scientists are unable to explain" lends support to the theory that plague will tend to break out more readily in regions where there has been severe environmental disturbance. Until recently the Delta has experienced only limited warfare and no major defoliation programs.

One major difference between the English plague and the Vietnam situation is that in England it was the English who changed their environment, while in Vietnam it is we who are doing so. We are therefore largely responsible for the potential consequences that may now affect a large segment of the population. The U.S. military can easily vaccinate its soldiers and a good part of the urban population, but what of the civilian village population, particularly in uncontrolled territory? According to Vietnam government statistics only about a quarter of South Vietnam's population has been immunized with plague vaccine.

OTHER DISEASES

Plague stands only as a model for what can happen when a natural setting is disturbed. Such water-borne diseases as typhoid and cholera, as well as various forms of dysentery and intestinal parasites, constitute a constant danger in Vietnam. Even malaria is sensitive to environmental changes that affect the breeding grounds of vector mosquitoes.

The ecological situation in Southeast Asia, as in any tropical area, is quite complex. This has two major implications for epidemiology: (1) The climate, both warm and humid, provides a benevolent setting for a wide range of infectious parasites. Organisms that would under other conditions have difficulty surviving outside of their human hosts remain viable for long periods in water supplies and in the soil. (2) Disruption of the ecological balance within either the settled or less densely populated areas can produce changes in both floral and faunal composition that, at the very least, are difficult, if not impossible, to anticipate. Some of these changes are bound to have their effect on epidemiological patterns.

WAR AND HEALTH

Extensive warfare in any part of the world has always had the side effect of producing severe impairment of public health facilities and of disrupting normal life patterns for civilian populations, which, in turn, are often correlated with rising disease rates. The imposition of modern warfare on Southeast Asia, particularly a war that is fought principally against guerrilla forces with no distinct battle lines, creates serious problems for the health of the entire population. Taking the tropical ecology and the type of warfare into account, the following factors can be expected to affect epidemiological patterns.

1. With vast areas of the country under only tenuous control, it is impossible to impose any effective public health measures on the population at large. Potential epidemics are difficult to check, and the endemic foci of disease cannot be wiped out.

2. Defoliation of forest land in guerrilla-held territory drives wild animal populations into new areas, increasing the risk of contact between animal reservoirs, potential domestic animal carriers, and human populations.

3. The dislocation and relocation of large segments of the population increase the possibility that groups from different ecological zones will cross-infect each other with exotic diseases against which they have no genetic or acquired immunity.

4. Both relocation camps and cities become overcrowded. Increased population densities and poor sanitation, combined with inadequate diets, increase the risk of epidemics.

5. Disruption of the local ecology and indigenous behavioral patterns leads to the breakdown of existing barriers to disease.

6. The stress associated with relocation, lowered nutritional standards, and cultural deprivation due to changes in traditional life patterns is sure to lower resistance to common diseases. Under such conditions, normally benign diseases can become serious health hazards for the population.

7. The increased disease risk produced in such groups may last long after a return to normal conditions. H. G. Wolf cited evidence that a prison-camp experience can have a striking effect on the continuing life chances of exposed individuals:

A recently completed study of the effects of imprisonment on Americans during World War II furnished revealing information about approximately 94,000 United States prisoners-of-war who were taken in Europe. These men were imprisoned for about ten months. Less than one per cent of them died before liberation. In contrast, in the Pacific theater, about 25,000 Americans became prisoners-of-war. They remained in prison four times as long as those captured in Europe, and suffered far more threats, abuse, and humiliation. Their demoralization was often extreme. More than one third of them died in prison.

Six years after liberation, the fate of those that survived the Japanese prison experience was investigated. In the first place, the total number of deaths in the group during these six years was more than twice the expected incidence for a similar group not so exposed and three times as great as the group of United States prisoners of war in Europe. Moreover, the causes of death included many diseases not directly related to confinement or starvation [italics mine]. Twice the expected number died of heart disease, more than twice the expected number of cancer, more than four

times the expected number of diseases of gastrointestinal tract. Twice the expected number died as a result of accidents. Nine times the expected number died of pulmonary tuberculosis. (J. M. Tauner, Ed. *Stress and Psychiatric Disorders*, the Proceedings of the Mental Health Research Fund 2nd Conference. Oxford: Blackwell's, 1958.)

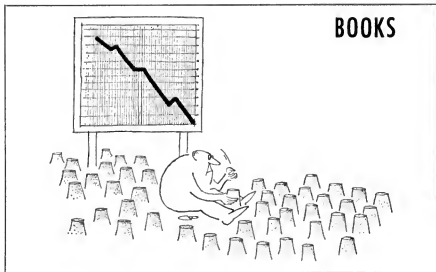
Now one might argue that the Vietnamese peasant relocated to camp or city has had a more rugged initial life experience and therefore is more immune to stress than his American contemporary. One might also argue that life in Vietnamese relocation camps is not comparable to life in Japanese prisoner-of-war camps. It must be admitted that we do not know what effects, if any, will appear in the incarcerated segment of the Vietnamese population several years from now, but published descriptions of the camps suggest that severe stress is an everyday experience.

It is therefore possible that we are risking the health of relocated and incarcerated individuals, possibly for a long time to come. Furthermore, this concerns a very large segment of the South Vietnamese population. Countless people have become refugees from American bombing or Vietcong attack. At least two million of these have been or are in relocation camps while the remainder have been left to their own devices. This is perhaps the first war in which a major segment of the civilian population has been so uprooted for so long. (One must include the Japanese occupation and the war with the French along with the present episode.)

8. The life risks of individuals not directly involved in relocation or evacuation may also be increased. Again according to Wolf:

The main point is that the great majority of the clusters of illness occurred in the lives of the members of every group (an experimental study of life stress in civilian populations) at times when they perceived their situations to be unsatisfying, threatening or over-demanding and productive of conflict, and they could make no satisfactory adaptation to these situations. These situations were in general those which arose out of disturbed relations with family members and important associates, threats to security and status, restrictions and limitations which made it impossible to satisfy important personal needs and appetites.

This kind of dislocation certainly occurs in all war, and even in normal circumstances, but the Vietnam conflict is particularly contributory. The new weapons used in Vietnam today, with the exception of defoliants, are merely an extension, albeit a terrible one, of "conventional" weaponry. But their vast destructive power, combined with our efficient, varied, and numerous delivery systems, used over and over again, year after year, is bound to have new and unhappy consequences for the entire ecology of Vietnam and for the majority of its civilian population.



THE VIRUS HOUSE: THE GERMAN ATOMIC BOMB PROJECT

EUGENE RABINOWITCH

The publication of The German Atomic Bomb by David Irving (New York: Simon and Schuster, 1968. Pp. 329. \$6.95) has promoted wide discussion in Germany and the United States.

Eugene Rabinowitch reviews the book in the first article which appears below. The Bulletin then reprints a review by Werner Heisenberg which appeared in the December 1967 issue of *Frankfurter Allgemeine Zeitung*; Professor Heisenberg was head of the "Virus-Haus" in Berlin-Dahlem. The last essay is by Hans Suess, professor of chemistry at the University of California, San Diego. Professor Suess appears in the book as one of the German scientists during the time under discussion; he offers his personal recollections of German science during World War II.

The story of the American atomic energy project during the Second World War was told, in considerable detail, immediately after the first use of the bomb in Hiroshima, in the so-called Smyth Report. Extensive details were provided in the official history of the project by David Hawkins (Manhattan District History: Project 4), published in 1945. The British told their side of the story a few

months after the Smyth Report. No full report on the Soviet wartime development was ever released; but many details were revealed in biographies of Igor Kurchatov, who had headed Soviet developments until his death in 1960 (Bulletin, December 1967). Still missing is a complete story of the German atomic energy efforts. Professor Goudsmit, scientific head of the American investigative team that went into Germany at the end of the war (together with, and sometimes ahead of, the troops), described his findings. The Alsos (Grove) mission removed from Germany not only its stocks of uranium and the most important German scientists involved in the project, but also all documents, reports, diaries, etc. on which the mission could lay its hands at Hechingen, Haigerloch, Munich, and other places to which German nuclear research had been evacuated in the last years of war. These documents remained stored at Oak Ridge and other U.S. archives until the British journalist, David Irving, located them and obtained permission to peruse them. He was given assistance by many German and other officials, with the notable exception of British authori-

ties who refused him access to the tapes of conversations between the ten German scientists (Heisenberg, Hahn, von Weizsäcker, and others), who were held incommunicado in a farmhouse in England when the first American bomb was dropped on Hiroshima.

The results of Irving's thorough studies of these documents are contained in an obviously reliable, blow-by-blow account of the German research, from its promising beginning in 1939 to a peak in 1942, and its stagnation and ultimate demise in 1943-45.

The account reveals that the worries of the American scientists (primarily of the European refugees among them) about a possible German success in producing the first atom bomb (which caused them to urge American efforts in this direction) were not exaggerated. In fact, until 1942 the Germans were ahead of the British and the Americans, both in the knowledge of essential scientific facts and in the scope of their experiments. The Einstein letter to Roosevelt on August 2, 1939, which became the starting point of the American project, was preceded, on April 24, 1939, by a letter to the German War Office from the Hamburg physical chemist, Paul Harteck, and his co-worker, W. Groth, suggesting the possibility of making nuclear explosives and pointing out the great advantage which would accrue to the nation having the first success in this enterprise.

The developments that followed were at first more rapid in Germany than in the United States or Britain, both in the reactor development and in the separation of uranium isotopes. These efforts were supported by the thousands of tons of uranium ore which fell into German hands when Belgium was overrun in May 1940, and by the German control over the only then-existing heavy water production plant in the world, occupied a month earlier in the German invasion of Norway. Assemblies of uranium or uranium oxide, in the form of plates or cubes, submerged in heavy water, were soon put together in the so-called Virus House in the Kaiser Wilhelm Institute complex in Berlin-Dahlem under Heisenberg, and in Leuna-Merseburg works under Har-

teck, ahead of the efforts of Fermi, Szilard, and Zinn in New York.

In 1942, however, the German project began to slow down, while the American one was rapidly expanding. The isotope separation research of Clusius, based on thermal diffusion, was unsuccessful, and no attempts were made to achieve isotope separation by effusion, was successfully carried out in America by Urey and co-workers in New York on the laboratory scale, and later in Oak Ridge on the production scale.

A fateful error in the determination of the neutron cross-section of carbon by Bothe stopped the Germans from experimenting with graphite as moderator in reactor assemblies. Instead, they put all reliance on heavy water as moderator; but just when the supply of heavy water from the Norwegian plant at Rjukan became ample, a decisive blow was inflicted by Norwegian parachutists, who blew up essential parts of this plant in February 1943. Attempts to resume heavy water production in Germany petered out, largely because of general disruption caused by allied air raids.

The attempts at electromagnetic separation of isotopes by Manfred von Ardenne in his underground laboratory on the outskirts of Berlin continued successfully, but its output was not large enough for the building of a U-235 bomb. Consolidation of all heavy water supplies in a single reactor project, under Heisenberg, twice evacuated and ending in a cavern near Haigerloch in Württemberg, permitted construction of an almost critical assembly. The work on this reactor was continued until the Allied troops were within a few miles. The German scientists nourished to the last minute the hope that if they succeeded in producing a self-sustaining nuclear reaction, they would give German science and German industry a tremendous boost after the clearly imminent military defeat. They did not suspect that, in the spring of 1945, Fermi's first self-sustaining nuclear reactor in Chicago was already over two years old, and giant plutonium-producing reactors were in full operation at Hanford. They were stunned, and at first couldn't believe it, when the explosion at Hiroshima revealed that the American development had progressed far beyond their own.

Another reason for the German failure, in addition to Bothe's error and the limited supply of heavy water, was the belief of German military leadership in rapid victory, and its consequent lack of interest in a project unlikely to pay military dividends within a year or two. This reluctance became particularly strong under conditions of economic disruption and manpower stringency which developed when the war continued into its third and fourth years. In Irving's interpretation, which American scientists who had worked on the Manhattan Project are unlikely to accept, another reason was that the American development was taken in hand by energetic military leadership, accustomed to operation on a large scale, while the German project remained to the end in the hands of scientists. They quarreled among themselves over limited supplies of uranium and heavy water, espoused different ideas concerning what could be achieved, and toward the end were interested primarily in the progress of nuclear physics under the guise of war research. Irving is particularly skeptical about the theoretical physicists, such as Heisenberg, von Weizsäcker, and others, compared to the more practical chemist such as Harteck, and inventors, such as Manfred von Ardenne. The German physicists surprised the military and political leaders by the modesty of their financial requests, which suggested to the leadership that they had little faith in their project. Paraphrasing Lloyd George's famous statement, Irving suggests that science in wartime is too important to be left to scientists!

While many of the American physicists on the Manhattan Project recognized the great organizational talent and drive of General Groves and his team of Army engineers, most of them complained that the military leadership had slowed down the development by its obsession with security (which proved ultimately ineffective). This was particularly true of the compartmentalization of research, enforced by General Groves, which impeded rapid exchange of information and ideas between the different laboratories. Only at Los Alamos, where the bomb itself was developed, did Oppenheimer succeed in preventing compartmentalization, permitting free discussion among all

members of the laboratory. The original drive and leadership on the Manhattan Project was provided not by experimentalists or engineers, but by theoretical physicists—Wigner, Oppenheimer, Szilard, Fermi (the last a unique master of both theory and experiment). (Physical chemists, such as Seaborg, Urey, and Spedding, also did play a very important role, particularly in the separation of plutonium from uranium.) While it is true that the gap between scientists on the one hand and industrial and military technicians on the other was wider in Germany than in America, few American scientists will "buy" Irving's conclusion that what was missing in the German project was efficient military leadership.

One last aspect of the German failure to develop an atom bomb in wartime has attracted considerable attention: alleged and lack of wholehearted support of their government by German scientists, and consequent reluctance to provide Hitler's regime with a terrible new weapon. This was asserted first by Robert Jungk in his bestseller, *Brighter Than a Thousand Suns*, and repeated by many German scientists, including Dr. Suess, a former co-worker of Harteck, in his criticism of the Irving book reprinted on page 36 of this issue. These doubts have caused German scientists to welcome the conclusion that atom bombs could not be developed during the war, thus avoiding the need to make a decision on whether or not to produce a bomb for Hitler, and to concentrate instead on the development of nuclear reactors as postwar sources of industrial energy. They caused Heisenberg to hint, in a conversation with Niels Bohr in Copenhagen in 1941, that if the Allied scientists would renounce the development of the atom bomb for the American government, this would strengthen German scientists in refraining from making one for the German government. However, the hint (if any) was so vague that it was not understood by Bohr, who soon after this conversation escaped from Denmark and as "Mr. Baker" joined the Los Alamos laboratory. Suess points out that Irving's book, although reliable and objective in telling the documentary story of the German project, could not convey properly the psychological situ-

ation in wartime Germany—the mistrust between scientists and the political leadership, and even between individual scientists, engendered by an all-pervasive totalitarian and intellectually incompetent dictatorship, and the consequent tendency to avoid responsibility for pushing the project ahead.

Undoubtedly, distrust and lingering doubts about the possible consequences of Hitler's Germany acquiring nuclear weapons did exist in German scientific circles, but it remained vague, inchoate, and altogether inadequate to absolve the German scientific community of participation in the great crime against humanity perpetrated in the name of the German nation by the fanatics of racism. Some German scientists were at first as eager to point out to Hitler's War Office the possible advantages of first acquisition of nuclear weapons as were Einstein, Szilard, and Wigner in America. In the minds of many, perhaps most, German scientists, the distrust of the Nazi regime was tempered by a vague hope that this regime represented only a temporary ugly face of a German national renaissance. The danger of military defeat to the future of the German nation overwhelmed, in their minds, the concern with the crimes of the then holders of national authority. For "unpolitical" German intellectuals, including some outstanding scientists, the Nazi movement, with all its crimes, represented a national revolution against a decaying, international, urban civilization, and a return to traditional moral values of the German race.

It was typical of German intellectuals that they avoided simple, pragmatic judgment of political realities by resorting to vague, general philosophizing, avoiding in this way the simple moral choice which confronted them in the face of Hitler's barbaric racism. Only a few, such as the great theoretical physicist, Max von Laue, the faithful friend of Einstein, escaped such philosophizing. Heisenberg was not among them; and some lesser minds saw in the expulsion of Jewish scholars a chance for faster advancement in the overcrowded German academic hierarchy.

When the war dragged on, and the likelihood of German defeat loomed more and more ominously for all who

retained a modicum of rationality, the scruples of the leading German physicists became stronger, and the alibi of developing not an atom bomb, but a postwar reactor, actually became reassuring.

Jungk's comparison of the hesitancy of leading German scientists to the eagerness of American scientists to push ahead the atomic bomb project, cannot be accepted as valid.

In the face of the fundamental difference between the (relatively) civilized governments of the West and

the cruel and barbarian regime in power in Germany, only a blame of a different order of magnitude can be laid at the feet of the American scientists. It lies in their failure to readjust their thinking when the danger of the German atom bomb was dispelled in 1945, and to rally a more effective opposition to the use of the American bomb on Japanese cities, attempted in the so-called "Franck Report" of June 1945. This report was not supported by the leading scientists of the Manhattan Project.

THE THIRD REICH AND THE ATOMIC BOMB

WERNER HEISENBERG

Virus-Haus was the cover name of a small laboratory in Berlin-Dahlem in which, at the beginning of World War II, the first German experimental research work on the utilization of atomic energy took place. David Irving uses this name as the title for a book describing in detail the German work in the field of atomic energy during the war. Around this work various legends were spun in later years, ranging from a suspicion that the atom bomb dropped on Hiroshima was a German-made bomb found by the Americans in the cellars of Haigerloch or Nordhausen, to the contrary assertion that German physicists committed high treason, keeping the knowledge of the atom bomb a secret from the Führer. One has to be grateful to the British historian, David Irving, for the trouble he has taken in devoting an extensive and time-consuming study to the sources of these legends and to a historically correct description of the events of that time. In this he has had the full support of German authorities and considerable support from Allied officials—considerable, but not complete. The informative tapes, recorded by means of secret wire-tapping devices, the conversations between the German nuclear physicists—held in 1945 as prisoners-of-war in England—after they were informed of the first atomic bomb drop over Hiroshima, have not been made available to him. In other respects, the book is supported by a much more profuse supply of sources, documents, pictures,

and conversations than was available even to the German authors of a report that appeared in 1946. Irving's description is fluently written and easily readable; many pictures enliven the text and memories of the atmosphere of that time. It is satisfying that Irving's investigation confirms the German report in all important points; beyond that it supplements and completes this report, presenting a clear and reasonably unbroken picture of the facts.

What are the most important facts? In the years 1939-42 the scientific basis for the technical application of atomic energy was laid in Germany, as well as in America. We learn from Irving that a few months after the beginning of the war, theoretical research had led to the conclusion that atomic reactors could probably be constructed by using, in addition to fuel made from natural uranium, deuterium oxide (heavy water), or pure graphite as a moderator to slow down fission neutrons. Furthermore, in such energy-liberating reactors a product will be formed from uranium (later named plutonium in America) that could be used as an explosive in atom bombs; and finally, that atom bombs could be built on the basis of a chain reaction with fast neutrons, if uranium enriched in the isotope 235 (the enrichment is technically an exceptionally difficult problem) or the just mentioned combustion product of the atomic reactor (plutonium), were to be used as an explosive. The experi-

mental research work carried out during the first years of the war gave these conjectures a solid foundation. According to Irving's careful description, the German physicists knew, in the summer of 1942, that the construction of atomic reactors from ordinary uranium and deuterium oxide (heavy water) was in fact possible. Incorrect measurement of the neutron absorption cross section of pure graphite did prevent the conclusion that this material, too, would be suited for the construction of reactors. Nevertheless, at that time, as Irving justly stresses, a method of constructing atom bombs was known in principle. The explosive plutonium could be produced in reactors with heavy water. A technically useful way of manufacturing the other nuclear explosive, uranium 235, had not been developed at that time. The German authorities responsible for armaments were, as Irving correctly reports, informed of these results at a conference in the Harnack-Haus in Berlin-Dahlem on June 4, 1942. However, no serious attempt to construct an atom bomb was ordered by German authorities after the Conference.

Subsequent experimental work, which Irving tells us never came to a complete standstill, amounted to running in place. At the end of the war, the results did not go much beyond those of 1942. The author records this fact without finding a sufficient explanation for it. The destruction of the only larger plant for the production of heavy water in Rjukan, Norway, first by a sabotage group and later by the British Air Force, hardly provides a sufficient explanation. No serious protection of this plant against attacks had been provided; also production of heavy water is not a very difficult technical problem, and it could have been undertaken in Germany.

In reality, the negative decision made by German authorities in the summer of 1942, which Irving does not adequately stress, was quite understandable and logical. At that time the war situation was already too tense for long-term technical projects. An order is supposed to have been issued prohibiting technical developments which would require more than half a year for completion. This situation spared the German physicists the decision whether to plead for an at-

tempt to produce atom bombs; they knew, on the basis of their technical experience, that such an attempt could not lead to success in less than three or four years. An attempt of this sort would have undoubtedly hastened German defeat, because the extensive manpower and materials necessary for it would have to be borrowed from other sources, thereby lessening the production of tanks and airplanes. Actually, in America, too, despite its much greater scientific and technical potential, in a country where the armaments industry could work without any disturbance from enemy air attacks, the production of atom bombs was completed only after the end of the European war. This fact was given too little consideration by Irving.

Indeed, at this point a weakness of Irving's book appears which is perhaps unavoidable. Wherever it is a question of facts, Irving's report, based on extensive studies of sources, is reliable and exact; but when it is a matter of motives, such documents are no longer sufficient. Here an intimate knowledge of the psychological situation in a totalitarian state at war would be necessary. Irving cannot possess this knowledge; acquiring it would be extremely difficult for a member of a Western democracy. Irving does not always escape the temptation to fill the gaps with stereotype ideas. He describes correctly the rivalries which existed between different German authorities—similar, by the way, to those that existed in America—but he does not recognize sufficiently how deep a mistrust could exist, yes, often has to exist, between human beings in a totalitarian state, even between those who work closely together. Misplaced confidence could be fatal. This some of our friends did in fact experience under the Third Reich. That at times we also inflicted bitter injustice through our distrust only became clear to us after the end of the Third Reich. Of this aspect of the situation Irving obviously knows but little.

By reading Irving's book one becomes aware of a peculiar, not always avoidable, danger connected with the temptation to misinterpret the psychological situation, and to resort to stereotype. When you read the book for the first time an impression can occasionally arise diametrically in contrast with the real factual content

of the book. This is demonstrated by a review that appeared in England, in which the essence of the book was not correctly understood.

In spite of such shortcomings it remains the great merit of the book that it presents the facts accurately and exactly. Whatever one may think about the motives, it remains a fact that a serious attempt to produce atom bombs in Germany was not undertaken, although in principle—but perhaps not in practice—the path to it had been open since 1942. But German physicists did not insist on pursuing, by means of practical measures, a path which could not have led to success during the war.

Whether Irving's book will be sufficient to destroy all hopes is doubtful. After a great war, history is written by the victors and legends develop which glorify them! Therefore, in spite of Irving's book, one can predict a long life for the legend of the "race for the atom bomb."

The above was translated from German by Margaret Seckel.

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VIRUS HOUSE: COMMENTS AND REMINISCENCES

HANS E. SUESS

"Germany's Atomic Research and Allied Counter-Measures" is the matter-of-fact subtitle of a book by David Irving which leaves one wondering how to define the very concept of historical truth. It engulfs the events of this period in a strange twilight, though much of the book is lucid and authentic, and long-forgotten details are recalled. Some of the most essential questions, however, are left in the dark, and misconceptions and misunderstandings are frequent.

It takes some time before one realizes why the book is wrong in many basic aspects and yet presents details with vivid truthfulness and unprecedented accuracy: the prime sources for the book have been German war files which, as the author states, "had been lying unused and neglected in a warehouse of the U.S. Atomic Energy Commission at Oak Ridge, Tennessee." Irving undertook the tedious task of piecing together from hundreds of documents a consistent story which, he then felt, was confirmed by numerous personal interviews with people who had lived in Germany during that time, and with many others who knew something about the course of events. For anyone who is familiar, because of personal experience, with the situation in Germany during the last war, this book demonstrates vividly all the tragic errors a historian can make if he derives his conclusions from the official documents of a totalitarian country. Irving was able to draw valid conclusions from German documents concerning scientific knowledge, ideas, and reasoning. But his views on anticipations, perspectives, and motivations are necessarily distorted and tend to correspond to the National-Socialist party line and to the official propaganda. Irving is a victim of the same kind of delusions that, shortly after the war, misled American investigators into believing that official Nazi files were better and more truthful sources of information than the testimony of reliable witnesses in such matters as the political conduct of individuals.

The following comments and additions are based on personal experience

and recollections. They are intended to help the historian reconstruct the prevailing atmosphere in which German atomic research was conducted and to learn what the motivations for Allied counter-measures during World War II might have been.

Scientists of all countries have the reputation of being nonconformists. Their psychology, both as individuals and in crowds, does not follow established rules. This was true, also, for the scientists working in Germany at the beginning of the war. Nothing could be more wrong than the assumption that they were guided by the eighteenth-century principle, attributed to U.S. Navy Captain Stephen Decatur, "My country, right or wrong." The majority of the German scientists were, at that time, far more critical of their government than the average American scientist ever was, or is, of his. As always, curiosity and ambition were the two chief motivations for scientific research, but at the beginning of the war a third was added: the desire to survive.

It is often believed that in the minds of many German scientists a conflict existed between their responsibilities toward humanity in general and their national responsibilities. This, however, was not true. Whether or not individual German scientists would have been willing to work on an atomic bomb for Hitler might be an interesting psychological question, but this question did not arise. As Werner Heisenberg repeatedly pointed out, the majority of the German scientists did not feel the need to make such a decision; they were convinced that their work would not lead to the development of a bomb, or to any other significant contribution to the German war effort. History has shown that this was true. Occasionally some doubt was expressed that the degree of confidence in this conviction was fully justifiable. That this confidence was warranted could be made plain and the many reasons which made the situation obvious to the unbiased observer could be listed. One of these reasons, the low war priority under which the German atomic research was conducted, is discussed below from the perspective of my own experience. There did exist, however, another type of conflict in the minds of many German scientists: the moral dilemma

created by the question of whether it was justifiable even to pretend to study the uses of atomic research for military purposes.

Political or military leaders are usually unable to judge the merits of research projects. This inability provided the opportunity, as some German scientists cynically put it, "to use the war in the services of science." To many scientists this attitude seemed justifiable, as a matter of self-preservation, in their dealings with their political superiors. It did, however, provoke a subconscious feeling of guilt and created the desire to justify and explain their actions to the international scientific community. It is possible that the discussion Werner Heisenberg had with Niels Bohr in October 1951 was instigated by such feelings. In any case, two of the most brilliant men of this century were unable to communicate their ideas to each other; their discussion resulted in a series of regrettable misunderstandings. It seems that even in the most objective and intelligent minds, war mentality can cause a communication barrier which, in some instances, has survived until the present, as can be seen from Irving's book.

This is what Irving says about Heisenberg's attempt in 1941 to discuss the German atomic energy research with Bohr:

"At the end of October, Heisenberg travelled to Denmark to see Professor Niels Bohr, to ask for his advice on the human issue. As Professor P. Jensen aptly put it, Heisenberg, the high priest of German theoretical physics, was going to seek Absolution from his Pope. Heisenberg asked the Danish physicist whether a physicist had the moral right to work on the problems of atomic bombs in wartime. Bohr countered with a question of his own: was the military exploitation of atomic fission possible, in Heisenberg's view, therefore? Heisenberg sadly replied that he now saw that it was. Heisenberg had intended asking Bohr whether he thought it feasible that all the scientists would agree not to direct the efforts of their governments toward the construction of atomic bombs, if he or they could be satisfied that the German physicists were also abstaining from such work. Unfortunately, Niels Bohr does not appear to have formulated his proposal as clearly as this.

"In any event, to Heisenberg's considerable amazement, Bohr replied that military research by physicists was inevitable everywhere and was thus proper too; he refused to be drawn into discussion on the German's proposal, apparently suspecting a German attempt at demolishing the feared American supremacy in nuclear physics, for which the enforced emigration of so many German physicists was largely to blame. Their conversation left a deep legacy of shock in Bohr, and a conviction that Germany was on the threshold of making an uranium bomb."

I feel certain that this account does not give the correct impression of Heisenberg's intentions, but I am certainly not in a position to describe them in an authentic manner. I only hope that my own recollections will help to identify possible misconceptions in Irving's account of the Bohr-Heisenberg discussions in 1941. Irving does not refer to Bohr's meeting, a few months later, with Hans Jensen, now also a Nobel Laureate for his work in theoretical physics.

To the conversation I had with Jomar Brun, the technical director of the Norwegian heavy water plant, early in 1942, shortly before he left Norway for England, Irving refers in the following way:

"Brun's conscience certainly was sorely troubled by the possibility that heavy water might be of military use after all. He had recently learned for the first time of the potential nuclear energy applications from an unguarded reference by Suess to a patent by Professor Joliot. Was this the reason why the Germans were at such pains to increase Vemork's production? Suess's remarks were reported to Tronstad in London: the German physical-chemist had tried to reassure him that the Reich's plans were of a peaceful nature, designed to aid Germany's post-war power economy. Suess had suggested that the development might take many years, but Brun was unconvinced."

Here, I feel that I am well qualified to give a detailed description of what actually happened. Perhaps this description will contribute an understanding of the general psychological situation which prevailed among scientists in German-occupied territory at that time, a situation quite differ-

ent from that inferred by Mr. Irving.

It was not easy for me to make the decision to risk discussing top secret German plans with Brun. Professional diplomats take a dim view of private interference with their work, and an unauthorized discussion of secret matters with a foreign national is considered an act of treason almost everywhere in the world. I was well aware that there might have been microphones in the room, or that Brun might have passed on my information to someone under surveillance by German spies. Moreover, there was a chance that Germany might have won the war and that I would then have been tried for treason on the basis of some captured Allied records.

Brun was primarily troubled by the possibility that the heavy water shipped to Germany might already be contributing to a German victory. Had there not been rumors about a new poison gas? About biological warfare? Brun was visibly relieved when I told him that the matter had no immediate application and that I was convinced that it would take many years. I guessed a minimum of five, before anything practical could result from the research which used the heavy water. "So then this is all Zukunfts-musik?" I remember Brun saying. We talked about the possibility of a nuclear chain reaction and the use of heavy water as a neutron moderator. Brun wondered why the Nazis were pursuing this long-range project. I replied that those who believed in a quick victory surely were hoping for its peaceful application after the war, and those who expected a long war were thinking that they must have some knowledge of all the possibilities that might result from such research.

I remember that I was talking freely in my attempt to explain, as convincingly as possible, my personal conviction that heavy water was not going to benefit the German efforts during the current war. I also remember the feeling of fear that everything I said could be held against me during a possible court martial for treason. Brun promised utmost discretion and said that he would carefully consider my safety should he have to pass on the information I had given him. A few weeks later he left for England. His report of my "unguarded" remarks, of peaceful plans of the Reich, etc., was

certainly influenced by this promise.

Many years later, Jomar Brun told me that he never had the slightest doubt that I had given him my honest opinion, to the best of my knowledge. However, he had not been entirely convinced of my competence to judge the situation realistically and he had thought that I might possibly be the victim of wishful thinking, or might have been deliberately misinformed in Germany. This, however, as we shall see, cannot have been the only reason why Brun's report in London did not have any noteworthy effect. Apparently the psychological communication problem that had prevented an understanding between German scientists and Niels Bohr during their discussion in Copenhagen also presented a difficulty for the Norwegians in their dealings with their British and American allies.

It should be easy to draw from existing German documents conclusive evidence on the type of war priority that was assigned in Germany to research on atomic energy. It had never surpassed the stage of "basic research" and its priority was ridiculously low. Probably no one high up in the Nazi hierarchy was willing to assume the responsibility of diverting an appreciable fraction of the industrial capacity to atomic weapons development as was being done in America by the Manhattan Project. In the event of a failure of such a project, Hitler certainly would have had its sponsors executed.

Irving shows very appropriately that the atomic scientists in Germany had mistakenly concluded, from their own inaccurate determinations of crucial nuclear data, that the possibility of getting a nuclear chain reaction going was solely dependent upon the supply of sufficient quantities of heavy water. My official position at that time was that of a scientific advisor to Norsk Hydro on matters of heavy water production. Following a suggestion by Paul Harteck and myself, it was planned to increase the heavy water production in Vemork by installing devices for deuterium exchange between the escaping hydrogen gas and the vapor of refluxing water. It was common knowledge that such exchange can be catalyzed by noble metals such as platinum and palladium. Such metals, however, were not made available for this purpose. Their total

amounts in the territories occupied by Germany were to be used for catalytic syntheses of all kinds, including that of ammonia in the nearby fertilizer plant in Rjukan. A quantity of several hundred grams of palladium would have been necessary in order to increase the heavy water production of the Vemork plant by a factor of two, but we were told that the only material that was both suitable as a catalyst and available for this purpose was a limited amount of nickel. In Hamburg, we had found that metallic nickel, dispersed on pumice stone, would have a sufficient catalytic property and that the effectiveness of such a catalyst was approximately proportional to the square root of the nickel concentration on the surface of the stones. This meant that the larger the volume of pumice stones used, the less nickel would be necessary to accomplish the exchange of deuterium in a given gas volume. It was, therefore, decided that huge drums, filled with pumice stone containing about five per cent of nickel, should be used. This was the result of having spent about six months of my time on tests and experimental measurements. This time could have been saved by making available a few hundred grams of palladium, or some hundred kilograms of nickel. This alone can serve as proof of the low priority assigned to the German atomic energy project.

It is possible that for some time the relationship between the priorities of the nitrogen fertilizer and the heavy water production was not clear to the directors of Norsk Hydro and that this was causing frequent confusion. The relationship became obvious, however, when, following the bombing of the Vemork plant in November 1943, the German Commissioner for Norway ordered the termination of any attempts to revive the heavy water production in order to avoid jeopardizing the production of nitrogen fertilizer.

The only counter-measures against German atomic research were three drastic steps intended to slow down and ultimately prevent this low-priority heavy water production in Norway: (1) a heroic act of sabotage against the high-concentration plant at Vemork in February 1943; (2) the bombing of Vemork in November 1943; and (3) the sinking of the Tinnsjö Ferry in February 1944. The

attack of November 1943 on the little village of Vemork and the hydro-gene plant nearby is well described by Irving. It was carried out from Britain by 140 Flying Fortresses of the Third Air Division of the American Eighth Air Force. The Norwegians had good reason to react as described by Irving:

"In London, the Norwegian government-in-exile had been shocked by the USAF raids on Rjukan: there had been no advance consultation with them at all. Nor, for that matter, had SOE (Special Operations Executive) been informed. On December 1, the Norwegians lodged a formal protest-note with the British and American governments, recalling their readiness to provide intelligence on Norwegian industrial installations and to initiate themselves the sabotage of vital war factories with the minimum loss of life and property in Norway. The damage caused by the bombing of Norwegian-Hydro's factories, first at Herøya, and now Rjukan and Vemork, seemed 'out of all proportion to the desired effect.' After the July bombing of Herøya, steps to improve cooperation between the Allied governments had been promised, as the Norwegians protested that they had already evolved plans for sabotaging their country's light-alloy production without crippling the associated fertilizer industry. Despite the promise of closer cooperation, however, the attacks on Vemork and Rjukan were executed without the Norwegians' prior consent, and without their even being informed. 'If,' the note continued, 'the reason for the attack was the necessity of stopping other production than that of fertilizers—for example heavy water—specialized methods of attack would have been more suitable than bombing.' The attack on the Telemark hydroelectric factories left a sour taste in many Norwegian mouths, and a deep-rooted suspicion that the raids were a product of American post-war commercial planning rather than of military necessity.

"It was a month before the British government replied: they rejected both the protest and the Norwegian suggestion that they were best suited to decide upon the fitness of targets for bombing or sabotage. Vemork had been bombed, the British note added, because accurate information had shown that the Germans had increased

security precautions to the extent where sabotage could no longer meet with success. Three weeks later, the Norwegian embassy in Washington was officially informed that the Secretary of Defense had assured the State Department that the most thorough investigation had preceded the bombings. The Norwegians' darkest suspicions seemed to be confirmed when the Americans refused to underwrite Swedish deliveries of the electrical equipment necessary for rebuilding the nonmilitary factories that had been bombed. Sweden sent the material, nevertheless, and within a few months the fertilizer and alloy factories were functioning as before."

Indeed, no satisfactory answer can be found as to why the Allies apparently had been so poorly informed about the state of German atomic research, why no serious attempts had been made to obtain more conclusive information, and why it was found necessary to take such drastic steps as the bombing of a plant owned by Norwegians and sacrificing the lives of numerous Allied soldiers and Norwegian civilians. The most unintelligible action of this kind was the sinking of the Tinnsjö Ferry at a time when German towns were already in ruins and German armies in full retreat. The sad background of this heroic action of sabotage was the following:

Foreign travel has its attractions. In wartime, a visit to a neutral country is a unique experience. Due to currency restrictions, the shopping list of Germans for the short stopover in Sweden, which early in the war had included diamond rings and other jewelry, was reduced in 1943 to such items as replacement buttons for overcoats, new shoelaces, and an occasional can of sardines. Also, the famous barrels of salted herrings were available in Norway. The absence of one of them in the baggage of a British agent on his way to Germany—so goes the story—gave him away. During the last month of 1943 the German war economy was gradually collapsing and no one in his right mind thought seriously of constructing a new heavy water plant in Germany. An order from the German Commissioner for Norway prohibited any attempt to produce heavy water in Norway, and for all those who had had some connection with the Norwegian project

there was no longer any reason to visit that country. The only excuse for one more trip to Norway was the possibility of selecting and securing the contents of electrolyzers in which the deuterium was sufficiently enriched so that it could be processed in Germany at some future time.

Permission for such a trip was granted to a few people, including me. None of us could possibly know that this trip would result in the senseless death of some 20 Norwegians who lost their lives when the Tinnsjø Ferry, Hydro, was sunk by Norwegian underground saboteurs. Irving describes this compellingly, but he leaves open the question of why it was still thought "very urgent" in London that the heavy water concentrates be destroyed. Was the sinking of the ferry a matter of exaggerated precaution? Did the sinking result from an administrative blunder, or from professional incompetence? Or was there some reason for this action which has not yet been revealed? It is clear from Irving's account that these are meaningful questions; we should know the answers.

It is difficult to understand why Allied Intelligence should have had so little reliable information regarding the state of German atomic research. A number of scientists associated with the German work, such as Rosbaud, myself, and several others, had volunteered some information. More conclusive information could have been obtained in several obvious ways. In 1942, for example, Bruun had suggested to British authorities that they contact me on my way through neutral Sweden at some suitable occasion. No such attempt was made prior to the bombing of Vemork. (See Per Bøhn, *Imi, Norsk Innsats i Kampen om Atomkraft*, 1946, page 62.) None of the research institutes engaged in atomic research in Germany were bombed or attacked in any way; all countermeasures were limited to actions in Norway, although without a detailed knowledge of Germany's plans, heavy water could by no means have been considered a necessary ingredient for the German project. I do not believe that there is any basis for the suspicion, mentioned by Irving, that "the raids were a product of American post-war commercial planning rather than of military necessity." More plausible than a commercial motivation seems

to me to be a political one, namely, the desire to impress atomic scientists working in America and their supporters. In general, it might have been felt that a knowledge of the true state of German atomic research might have adversely affected their enthusiasm for the Manhattan Project.

I have enjoyed discussing all this with Mr. Irving personally, although we could not agree upon the relative values of official documents as compared to the testimony of contemporary witnesses. I appreciate the immense difficulties faced by the historians who wish to reconstruct in an objective manner developments and episodes that have occurred in totalitarian countries. In my opinion, so long as contemporary witnesses remain alive

the true story about German atomic research during the last war can still be prevented from being lost forever.

For anyone, but especially for the readers who have some technical knowledge in the field of atomic sciences, Irving's *Virus House* provides fascinating reading, and a wealth of valuable information as presented in the records. But for an understanding of the psychological situation under which Germany's atomic research was carried out during the last war, and probably also for the understanding of the motivations of Allied countermeasures, the book does not provide a satisfactory background.

The author would like to note that the above reminiscences were written independently of Heisenberg's review.

BEYOND VIETNAM: THE UNITED STATES AND ASIA

By Edwin O. Reischauer. New York: Alfred A. Knopf, Inc. Pp. 242. 1967. \$4.95.

Reviewed by
RICHARD H. SOLOMON

America's distinguished ambassador to Japan from 1961 to 1966 has written a book which finally develops a sense of historical proportion and regional perspective on our involvement in Vietnam. Unlike the mass media, which daily blast us with a soldier's eye view of the war, or numerous recent polemical books and articles which attack the American involvement in this sliver of a country in terms largely isolated from larger security matters, Professor Reischauer has attempted to place the puzzle of the Vietnam war in the surrounding jigsaw map of regional and world problems.

"This is not a book about Vietnam," are his opening words, yet each chapter brings us back to this nagging contemporary problem just as a missing piece in a puzzle is conspicuous by its vacant outline. Reischauer reviews how America's post-World War II reinvolverment in Asian security tasks proceeded according to the largely misleading experience of our successful Cold War aid and military commitments in Europe. He details America's strange and enduring preoccupation with China—an emphasis out of all balance with our "real" economic interests which have continued to reside in Japan. He seeks to redress

our perspective on Asia by mapping the serious imbalance between population and per capita resources in this part of the world, and appropriately identifies the real limits of Chinese power. The author, an expert on Japan, is not being partisan in his efforts to shift America's gaze away from the "Chinese threat" to the region's major power, Japan—an emphasis that seems quite appropriate to this "Chinese" reviewer.

The Vietnam piece in the larger Asian puzzle is most conspicuous when the author relates our present unhappy entanglement to likely future problems. He asserts that, next to a nuclear war, "the greatest danger of the present situation is that . . . the pain of the experience [of Vietnam] may induce Americans to withdraw from Asia into [a] sort of Occident-centered neo-isolationism." His most convincing arguments, paradoxically, are largely supportive of an isolationist conclusion: "We in North America would have the least to worry about [Chinese domination of Asia]. The United States is too rich and strong and so far away. . . . We would not be threatened by the kind of military power Asia could develop." And if America should abandon a "stabilizing role" in Asia, "next to the countries

of that area, Japan would be the chief loser."

The arguments Professor Reischauer marshals in support of an "involvement" position are perhaps less convincing because of their conditional and future nature, as opposed to the present costs of the war. He asserts that as part of a unitary world, an Asia which did not evolve toward "peace, stability, and prosperity" through American help would in time present us with regional conflicts which could spread to a world holocaust. Our interest is primarily in Asia's future, in her large population and the capacity of her "industrious people with millennia of high civilization behind them" to affect our lives more directly in 20 or 50 years. And finally, the author sees "simply our own moral imperative to help those who need help."

If the record of the past is any guide to the likely future of American efforts to aid the countries of Asia in their national development, the prospects do not seem bright. The author traces how our present involvement in Vietnam grew from the incremental advance of almost casual decisions until, "bit by bit our commitments in Vietnam built up to the high stakes that now would make our withdrawal and acceptance of defeat so extremely costly." He places the blame for this unwitting involvement on a "grievous misreading of Asian history" by our leaders, and a foreign policymaking process which forces them to "jump from crisis to crisis, thus staying behind our problems as they develop rather than ahead of them." And he notes with frustration America's distorted national priorities, in which billions of dollars are spent on space exploration or intelligence activity, in contrast to a foreign policy formulating process in which "not a hundredth as many people or as much money is devoted to the formulation of the policy itself."

If our governmental instruments of foreign involvement have their limitations, it is the Asian political environment, as so effectively sketched out in this book, which raises the greatest problems for effective American action in the developing world. Our economic wealth, as transmitted through aid programs, is as likely to perpetuate elitist political groups as it

is to filter down through local governmental bureaucracies to reach the people who might effectively use our technology or capital. Our massive military presence can demoralize local elites, who are all too happy to see others bear the burdens of defense while they attend to more pressing matters of political relationships. And the American presence in one country of the underdeveloped world can lead to our entanglement in regional rivalries, or provoke the intervention of great powers doubtful of our motives, thus producing the kinds of confrontations or conflicts we seek to avoid.

Professor Reischauer suggests guidelines by which to resolve these perplexities: "Any regime that is not strong enough to defend itself against its internal enemies probably could not be defended by us either and may not be worth defending anyway"; and he urges the internationalization of our aid efforts.

In overview, however, this book leaves one with a great sense of unease, for the author develops few convincing rational arguments—as opposed to a "gut" imperative to at least make an effort—for continued involvement in Asia, either in terms of our capacity to be effective in an aid role, or in the context of defense against a significant Chinese threat. He asserts that "we should maintain substantial military power in the Western Pacific in

order to deter aggression," yet concludes that even China will "not be a military threat but a more subtle one . . . of incitement to subversion, and of support to insurgency." Yet by Reischauer's own analysis, it is the inappropriateness of our overwhelming military response to a "national" political conflict in Vietnam that is the root of our present dilemma. Does this advice not set us up for future Vietnams?

In sum, *Beyond Vietnam* suggests that the frustrations and puzzlements America faces at this hour are but part of the larger map of political and social change in the underdeveloped countries of Asia; and that given any likely outcome of this present conflict, Americans are certain to face political choices of abstention or intervention in underdeveloped areas where the risks and costs are as great as those we bear today. Professor Reischauer has made an important contribution to our public debate by spelling out the dimensions of this political environment and the paradoxes the United States faces, even if his analysis of the necessity or the capacity for effective American involvement in this part of the world is less than convincing.

Richard H. Solomon is an assistant professor of political science and a member of the Center for Chinese Studies at the University of Michigan.

FURTHER COMMENTS ON REISCHAUER AND THE CHOICE ON THE WAR

NATHAN LEITES

While Edwin Reischauer begins his remarkable analysis by indicating that "this is not a book about Vietnam," it is only on that country that I shall follow him here, and only on points or formulations with which I disagree. To them (designated by "R") I shall present alternatives ("A"), without being able to show in these pages why evidence seems to me, on balance, to support the latter. Like Reischauer, "I put forward these . . . views . . . with some diffidence, because I am not an expert either on Vietnam or on military matters" (pages 18-19).

R: It "seems highly probable" to Reischauer that a Vietnam in which

the Communist Party had consolidated the control it had won in the summer of 1945 (rather than, first, losing much of it) would have developed a "relation to China not unlike that of Tito's Yugoslavia towards the Soviet Union"; also because "Ho, like Tito, had had cordial relations with us" (page 30).

A: Mr. Reischauer's alternative history seems highly probable to me, too. But the particular factor quoted last—whatever "cordial" here means—appears to me unrelated both to the conjectured course of events in Asia and to the actual development in Europe.

R: A Vietnam under Communist

control from 1945 on would have been "paying lip service" to China (page 31).

A: Even that is not too probable. There is no traditional dogma in Asian communism affirming the Chinese Party's superiority; and the Vietnamese Party probably feels its record—including a seizure of power in its entire country at a moment when the Chinese still seemed far from that level of success—to be at least as impressive as that of its Northern neighbor against whom no foreigner has intervened since 1945.

R: In 1945 Ho "apparently" expected our continued "friendship" (page 30).

A: Ho does not seriously conceive of "friendship" with the leading state of the non "socialist" world. No evidence is available which would allow us to guess what Ho at the end of World War II expected the course of his relations with Washington to be. He had more to "hope" for in economic aid from us than from China (*ibid*). Our capacity for aid was larger, but what about our intention, actual or predicted by Ho?

R: "The United States . . . gave no indication at Geneva [in 1954] that it would oppose the elections [scheduled for 1956 to decide on the unification of Vietnam]" (page 24).

A: It was hard to believe, and was little believed, that the United States would do anything but "oppose" them if a date were ever set: the majority of the voters (in the North) would have been under totalitarian control. That such elections "would throw the South into the hands of the Vietminh" was not an "assumption" (page 25), but a certainty. To say that "our greatest and most obvious blunder was not to support the Geneva agreements" in this regard (page 31) is to assert it to be a grave mistake not to vindicate the totalitarian practice of elections by American "support."

R: The leadership of the Vietnamese Communist Party, ruling the North from the summer of 1954 on, was "outraged" by the "flouting" of the Geneva agreements with regard to elections (page 26).

A: The moment any such leader sensed in himself any attachment to *pacta sunt servanda*, or any depth of outrage about anything an enemy does, he would feel shattered about

his own "degeneration." The leaders in question did not expect Diem, or anybody else attempting to rule from Saigon, to aspire to ceremonial suicide by pressing for elections, about which they themselves were, to borrow Mr. Reischauer's words about these years, "curiously passive" (pages 26-27).

R: A consequence of our "underwriting . . . [Diem's] refusal to go through with the unification of Vietnam" was that the Party's (I shall use this word to designate the Vietnamese Communist Party) "animosity towards us increased" (page 31, my emphasis).

A: Our support of Diem's refusal to transfer the South to the Party's domain by the election mentioned in the Geneva agreements increased the Party's expectation of a conflict with us.

R: One component of the Southern rebellion in the later fifties was "organized" activity by "Southern Vietminh" (page 27).

A: The dominant contribution to the insurgents' organization was and is furnished by the Party, which maintains much centralization despite the country's division, and whose leadership is "Northern" mainly in the sense

that national headquarters are in the North where the Party rules. Much of the "organized" activity by "Southern Vietminh" in the later fifties is likely to have been in conformity with policy set by the Central Committee of the all-national Party.

R: The return to the South of Southern Vietminh who had gone North after the Geneva agreements began after the announcement, in late 1960, that a "National Liberation Front" had been founded in the South (page 27).

A: It began before.

THE TWO REGIMES

R: After the Party's conquest of power in the North its policies produced "agrarian unrest" (page 27).

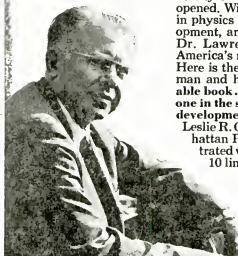
A: At that time the Party destroyed—fully, in an economic sense, and to a considerable extent, in a physical sense—the well-to-do farmers, with a severity at least equaling, say, the "liquidation of the Kulaks as a class" in the Soviet Union of the late twenties.

R: In May 1963 a "serious" Buddhist "uprising" broke out in the South, and in the spring of 1966 a

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"large scale" "uprising" (pages 27-28).

A: These were less than what is ordinarily meant by an uprising.

R: Diem's suppression of Vietnam remnant and of "all open opponents" was "ruthless"; it made "something of a police state" of the South (page 26).

A: If these (proper) words be chosen to describe Diem's conduct, other terms should perhaps be selected to designate the higher level of threats raised and damage inflicted by the Party, before and after conquest of power; and I have missed in Mr. Reischauer's account a sentence which would be a full counterpart to the one quoted. When he speaks of the Party's "dictatorial, oppressive" rule in the North (page 29), the reader little informed about Vietnam may gain the impression that in the author's view North and South are coercing their citizens to about the same degree. Indeed, Mr. Reischauer follows his conjecture that a Vietnam under the Party's control since 1945 "would probably not have been something we would have approved of" with the reminder that "we have not found much we could approve of" in the South either (page 29).

Vietnamese Balance of Power

R: "People usually discuss what Hanoi or Washington might be willing to concede, and what pressures Peking and Moscow may bring." However, "the problem lies with the two . . . protagonists in the war: the supporters of Saigon and . . . the National Liberation Front"; on the one side the Vietcong, and on the other "the South Vietnamese military establishment and a congeries of quarreling religious bodies and political factions," which are, however, "held together . . . by a determination not to fall under Communist rule" (page 8).

A: On the one side there is the Party, in power in the North; in the South, presenting itself and operating largely through its front, the Front.

In the congeries on the other side, the resolve to fight each other seems to surpass whatever "determination" there is to contribute to the prevention of the Party's conquest of power in the South. Vietnam is the only country in the world which remained non-Communist after 1945 solely to the extent to which the threatened or actual presence of foreign soldiers suf-

ficed for this purpose. As Reischauer himself puts it in another context, "all along . . . probably the only . . . alternative to what has happened . . . was to allow Ho . . . to take over the whole of Vietnam" (page 28).

Termination Through Negotiation?

R: The bombing of the North "may so build up" the "hatred and distrust" of "the North Vietnamese" toward the United States that it "increases their determination to go on fighting," reduces their "willingness to negotiate" (page 6).

A: The particular North Vietnamese who, under whatever influence from the people at large, make the decision to negotiate or not, are the leaders of the Party. They are apt to view it as a grievous mistake—as many other less austere politicians also do—not to entertain extreme distrust of enemies at all times, and as a sacred obligation to transform hatred of the enemy into conduct which will reduce as much as feasible losses at his hand, enhance as much as possible advance at his expense. Of course, they do not always succeed in disciplining emotions, but the record shows that they are apt to be as brutal toward their own souls as against enemies.

R: One policy is to "go on fighting on somewhat the present terms, in the hope that in time we could bring about a deescalation . . . and . . . persuade the Vietcong and Hanoi to seek a settlement. . ." (page 9).

A: While it is possible to indicate conditions in which the leadership of the Party might decide to deescalate, it is harder to imagine situations in which they would prefer explicit self-limitation to temporary reduction in activity—unless they came to believe that the former would decisively weaken Saigon (in which case it might not be acceptable to us) or furnish a pretext for an American withdrawal. But the acquisition of such insight by the Party's leaders is obstructed by their horror of yielding beyond present necessity, as well as by their certainty that only pressure works.

R: "I wonder if any sort of agreement will ever be reached until one side or the other recognizes that it faces eventual defeat" (page 8).

A: Particularly then each side may

prefer unadmitted (where feasible) or properly embellished withdrawal to formal renunciation, or (for us) the appearance of "peace with honor" soon to be followed by utter failure.

R: After the American elections of 1968, the Party may be "more inclined" toward a negotiated peace if it is given, in the South, a "tolerable" alternative to an "apparently endless" war (page 18).

A: With dedication, courage, and skill, the Party has been fighting for more than 20 years; there were interruptions, to be sure, but while they lasted, the Party looked toward the resumption of the struggle. Though the trend has been in its favor, there have been lengthy phases of stagnation and regression; the leaders expect them to recur, and have not ceased molding themselves and the cadres to be tenacious in stalemate and adversity.

At least until the early seventies, therefore, the Party is more apt to decide in favor of lying low in the South—as it did from the middle to the later fifties—than to reconvert itself so as to operate in an alien legality, supposing Saigon were to offer it a genuine chance of that kind.

Escalation

R: Attacking on the ground in the North "would probably only mean . . . that we would have two guerrilla wars on our hands. . ." (page 10).

A: Worse. Even if we succeeded, with a much enlarged investment cost, in depleting our opponent's armed forces in both parts of the country, induced him to disperse into small units and to avoid encounters with us—if we were as successful as the French in Algeria, 1959-61—the maintenance cost of this result would probably include a protracted continuation of our massive military presence. This kind of forecast made de Gaulle, in late 1960, decide upon "disengagement" from Algeria.

Withdrawal

R: The "counterstrategy" of building a "more tenable defense" (against the expansion of the zone where Communists rule in Asia) in "sounder terrain," such as Thailand, would mean "the further spread of American military power into areas where the Vietnam war had just shown that our

type of military power was . . . ineffective" (page 13).

A: The withdrawal would have been caused not by what Vietnam and, say, Thailand have in common—being in Southeast Asia—but rather by what has made a difference between them for more than three decades: the Vietnamese Party's performance—the level of political and military energy generated by it per capita of cadres and country—is among the highest in the world; that of the Thai Party, insignificant. On the other hand, those opposed to the Party in Bangkok have been less feeble, and less prone to consume their forces in struggles among themselves than has been the case in Saigon.

R: Withdrawing from Vietnam would be "welching" on a commitment.

A: Yes, as to the texts of assurances given. No, with regard to a rarely stressed, but hardly hidden, condition never clearly renounced by Washington: the enterprise of preventing the Party's conquest of power by arms in a country should be a genuinely common enterprise of its anticommunists and their foreign allies. That the internal resistance against the Party might be and might remain feeble was hardly envisaged in the American mainstream until recently. That is the case of Vietnam, familiar today, but unexpected by those who initiated and enlarged American intervention between the mid-fifties and the mid-sixties. Imagine a weird world: a temporarily cohesive, impressively large, reasonably dedicated, and moderately efficient Freedom Front, capital Hué, utilizing much of its human and material resources in its civil war—and insisting on our doing the rest, urging us to destroy what must be, as, to a noticeable extent, the occupied Europeans did a quarter of a century ago—that is the "doctrine" of "containment" applied to Vietnam. At no point has it been resolved, at any level in the United States, that we assume henceforth the obligation to endeavor, by our own force, stopping the Communist Party in a country where the balance of the will and skill to believe and persuade, to coerce and suffer, to kill and die, is decisively in its favor.

Nathan Leites is a professor of political science at the University of Chicago.

CHEMICAL ABSTRACTS SERVICE ANNUAL REPORT TO NSF

EUGENE GARFIELD

In the Spring of 1965, the National Science Foundation (NSF), acting as coordinator for several government agencies, signed a contract with the American Chemical Society (ACS) to develop and test a registry file of chemical compounds and to conduct selected research and development on chemical data handling. The Chemical Society has now reported on the first year of what originally was to be a two-year program costing the taxpayers over \$2 million. Already it appears that consideration is being given to escalating the project in duration and size at a cost of \$20 million.

Whether or not the government continues to fund this project is of vital concern to the entire scientific community—not just to the relatively small number of industrial and government chemical organizations who will make the greatest use of this system when and if it comes into operation.

The project and the report should raise a broad range of questions—political, scientific, and economic—for serious consideration by the scientific community and the public at large.

When the federal government subsidizes a new scientific information activity, the scientific and lay communities should be alert because government subsidies are always accompanied by government controls. The degree of control which the government chooses to exercise at any particular moment may be unpredictable but recent history is replete with instances of ill-advised and even silly restrictions placed on the free flow of scientific information in the name of national security.

If, however, the government claims that it does not intend to control, but merely to support, an information program, the citizen may rightly ask by what authority it grants a particular private organization a virtual monop-

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request.

only on a vital source of information. This criticism was anticipated in intragovernmental discussions which preceded the award of this contract to the American Chemical Society. That the particular private organization happens to be a professional society with a tax-free status, will not and should not pacify congressional critics, since the ACS, like other professional societies, is designed to further the self-interest of its members rather than that of the American taxpayer. Indeed, it may properly be asked why the government should subsidize the activities of a wealthy professional special-interest group when that group discriminates against public libraries and other "non-members" through different prices charged for its publications and services.

Why was this large contract awarded without conducting a simple and inexpensive market survey to determine who needs the "product" and what they would be willing to pay for it? Whenever the chemical notation systems are discussed at scientific meetings most chemists appear uninterested. Yet, the same small clique of notation enthusiasts shows up—representing industry, government, and documentalists—with perhaps only a dozen or so persons seriously involved. This does not mean that the benefits of a chemical registry and structure searching system will not ultimately benefit thousands of chemists throughout the world, in the same way that mankind may benefit from a hundred million dollar particle accelerator even though only a few may use it. It is rather a question of national priorities.

Criticism also comes from private entrepreneurs in the growing information industry. Such critics ask why government agencies completely bypassed established government procedures in the procurement of this multi-million dollar "research" program. The question is made even more pertinent by the fact that the program is more development than research since the answers to many of the "questions" spelled out in such detail in this report were available years ago from studies of the files of the Chemical-Biological Coordination Center.

The project shows no respect for

well-established statistical sampling techniques. Apparently the project managers feel that the only way to test their system is to build it first and try it later. Having planned on the encoding of 200,000 chemical compounds, the project has done closer to 800,000 and appears to be headed, in the already signed follow-up contract, toward at least doubling that figure. Where does one stop before deciding the system works? The elemental composition of chemical compounds has not changed much since the CBCC analyzed them, and the CBCC frequency distributions are particularly pertinent since Chemical Abstracts (C.A.) plans to go back and pick up several million old compounds.

A number of small developmental studies have been conducted which seem useful, such as comparing magnetic tape typewriters to key punch machines and typewriters for the input of information on chemical structures to a computer. But no one at C.A. seems to have asked why one should spend from one to two dollars per compound to get a structure into the file when it could be done at lower cost using the already tested Wiswesser notation system or a similar system. If one has to draw a structural diagram in order to "encode," then this will be the same in either system. But this requirement happens to be built into the C.A. system because the abstractors have converted diagrams reported by authors into nomenclature. If the encoding were done directly from the original documents or from similar sources containing the structural diagrams, it would be absurd to expect to use character readers in the near future. If one has the structure or a systematic name, then presumably algorithms exist or can be created which would convert systematic notations to connection tables or any other atom-by-atom connection one needs or desires. C.A. is almost obsessed with the objective of producing these connection tables as a by-product of a structure drawing operation. All this seems irrational if Chemical Abstracts is to be the source of the information.

The fact is that structural diagrams or their equivalents must exist in the original documents that will be retrieved in the registry system. There-

fore, much of the activity described in this report is in a vicious circle. Given the structural diagram, it appears to be economical, according to notation enthusiasts, to create a line notation. If the diagram is not given, then a name must be given; else where is the compound reported? Given the chemical name, one could also create algorithms for converting nomenclature to whatever canonical forms one desires. That the present report skirts these fundamental issues derives from the political nature of the controversy that has raged between the C.A. Dyson-International Union of Pure and Applied Chemistry enthusiasts and the Wiswesser enthusiasts. The situation would seem less political if the National Science Foundation or the Office of Science and Technology simultaneously had supported studies by both camps but, given the blessing of a National Academy of Sciences ad hoc committee that C.A. is the logical organization to "do" chemical notation things, accompanied by a government obsession to centralize all chemical information activities somewhere, the result was inevitable—a unilateral approach which is subject to many serious scientific and political objections.

Finally, the free dissemination of scientific information is already in jeopardy through the structure of this C.A. project. An important appendix discusses the means by which C.A. intends to retain the confidential nature of data included in its files. In addition to data on compounds reported in Chemical Abstracts itself, data from such government agencies as the National Cancer Institute and, one presumes, also the chemical-biological warfare branch of the Army, among others, will be tied into this system, but the compounds will not be accessible to those who do not have a right to such information. In brief, the premises of C.A. will now contain information which will be rated as "confidential," and one wonders when security guards and clearance procedures will be instituted in the new \$8 million C.A. edifice at Columbus, Ohio, for Chemical Society members and "non-members" alike.

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SCIENCE, SOCIETY, AND SCIENCE EDUCATION

GEORGE BASALLA

Science emerged at the end of World War II as a new but widely recognized force in American life. Even in that pre-Sputnik era some educators and scientists, aware of the national importance of science and dissatisfied with its teaching on a high school level, were laying plans for a long-delayed reform of secondary school science. Then, in October 1957, the Soviet Union formally opened the space age, bringing new and powerful support to the reformers of American science education.

The first of the major reform programs was undertaken in the spring of 1956 at the Massachusetts Institute of Technology by the physicists and science teachers who formed the Physical Science Study Committee (PSSC). Reacting against the traditional physics courses, which stressed rote learning and practical application, the reformers organized their course around the basic concepts of modern physics. The PSSC course was carefully designed to stimulate the student's interest in physics by presenting it as a "continuing process by which men seek to understand the nature of the physical world."

Shortly after work began on PSSC physics, the Chemical Bond Approach (CBA) Project initiated a revision of the teaching of chemistry in the secondary schools. Early in 1960 the Chemical Education Materials (CHEM) Study group further en-

larged the reform movement by the addition of its program—stressing the experimental side of chemistry—to the new high school science curricula.

The task of reforming secondary school biology dates back to January 1959 when the American Institute of Biological Sciences created the Biological Curriculum Study Committee (BSCS). Acknowledging the diversity inherent in the biological sciences, BSCS gave high school teachers the choice of three approaches to biology in its revised curriculum. The "blue" version of the new textbook took molecular biology as its theme, the "yellow," cellular biology, and the "green," ecology.

In the teaching of physics, chemistry, or biology, reforms followed a similar pattern. New curricula were created by teams of scientists, educators, and technicians who worked to incorporate recent developments in the sciences into a unified course built around fundamental themes or viewpoints. New textbooks, most of them collaborative efforts, were released only after they had been extensively tested in selected classrooms across the country. Old experiments were discarded and new ones created to insure maximum student participation and comprehension. Traditional laboratory apparatus was replaced by simpler pieces designed to reveal, not conceal, their mode of operation. Visual aids—including films, slides, models, and charts—were integrated with other methods. Institutes and meetings were held to instruct teachers concerning proper use of the new material. The

net effect was to make high school science meaningful and exciting for many students.

The era of reform has not ended; the creators of science curricula are not satisfied that they have produced the ideal course in any single field of science. The current work of the reformers can be seen in the recent Report of the International Clearinghouse on Science and Mathematics Curricular Developments (Ed., J. D. Lockard, AAAS and University of Maryland, 1967). Here are found over 80 projects covering kindergarten to grade 12 and including a major effort to revamp high school physics: Harvard's Project Physics. Since we are in the midst of a continuing revolution it might serve a useful purpose to examine critically dominant trends in the field and offer an alternative basis for teaching high school science.

THE NATURE OF SCIENCE

Despite the revolutionary character of the new science curricula, they share a common weakness with their older counterparts: concentration on the end-product of science. The ultimate concern is with content, not process; with scientific knowledge relating to the physical universe and not with the revelation of the nature of the scientific enterprise to young students. In all the new textbooks there is a short, introductory section devoted to a discussion of scientific inquiry; but few, if any, of the works extend this discussion throughout the volume to make it a consistent and fundamental thread in the fabric of the course. In every case, the enlightened textbook authors are more interested in moving on to new scientific topics, reorganizing older materials, and devising new and ingenious experiments and apparatus. Therefore, students exposed to new science curricula learn very little about the nature of scientific inquiry. They never learn, to quote a distinguished educator, Joseph J. Schwab of the University of Chicago, that science is more than a "rhetoric of conclusions," that it is "... a mode of investigation which rests on conceptual innovation, proceeds through uncertainty and failure, and eventuates in knowledge which is contingent, dubitable, and hard to come by" (*The Teaching of Science as Enquiry*, Harvard University, 1962).

"Uncertainty in Science" is a topic covered in the opening chapter of the CHEM Study textbook, but it is not the uncertainty that Schwab had in mind. It is rather the lack of certainty in measurement, and the topic developed in the text is the importance of significant figures. No one would dispute the authors' wisdom in emphasizing the necessary error in measurement for introductory students of chemistry. Still, it is disconcerting to find in bold-face type the sentence: "Every scientific statement involves some uncertainty," followed immediately by a reminder that this uncertainty is best indicated by the symbol $\pm$. Surely there would have been a natural place to expand the topic to include some remarks on the provisional nature of scientific knowledge, its inherent strengths and weaknesses. The failure to do so can be attributed to the viewpoint of too many science educators; for them, the uncertainty of scientific knowledge is to be related exclusively to matters that can be handled in a routine fashion. The image of science as a revealer of ultimate truths is thus maintained.

THE NATURE OF TECHNOLOGY

Not only do the new curricula fail to clarify the nature of science for the students; they also fail to discuss the nature of technology and its relationship to science. Technology is associated with engineering and practical application, two fields that run counter to the general philosophy of the reformed science programs. Revolving against the older courses in which physics students studied the operations of doorbells and refrigerators while biology students received elementary lessons in disease prevention and nutrition, the reformers embraced the theoretical, the "pure" side of science. A balance is not to be achieved by returning to the illustration of scientific principles through the workings of household appliances but there should be a place in the new curricula for the serious study of technology, applied science, research and development, and associated topics. We can surely expect that a student in the twentieth century should be taught the significance of technology: its emergence with mankind, its relatively recent interaction with science, and its transformation of the economic lives

of a large majority of the world's peoples.

SOCIAL IMPACT OF SCIENCE AND TECHNOLOGY

The reformers of secondary school science, scarcely interested in the student's exploration of the nature of science and technology, are even less concerned that he study the great impact science and technology have had upon society. Science and technology created modern society, radically transformed it within a short time, and now threaten to annihilate man—or at least poison his environment and disrupt social intercourse—if they are not properly channeled. Yet students of high school science learn virtually nothing about social consequences of modern science and technology. In 1961, the National Science Teachers Association issued a report, *Planning for Excellence in High School Science*, stating: "As a result of science education, students should recognize and accept their place in a society which is largely scientific in character." After six years, little has been done to develop this suggestion.

The predominant roles played by the physical sciences in shaping new science courses have resulted in neglect of the social sciences and perpetuation of the invidious distinction between "hard," laboratory sciences and "soft," social sciences. This is especially unfortunate at a time when we need the help of the social scientists in understanding and solving the social problems generated by scientific and technological advances.

At an early age the student comes to identify scientific activity with laboratory experimentation. It is then that he lays the basis for a life-long prejudice against the non-experimental social sciences. The discussion of scientific activity he is likely to encounter in his science textbooks does nothing to dispel this prejudice. The CBA Project volume, for example, bypasses the complexities of the question "What is science?" by defining science as the work carried on by Lord Rutherford! Such a definition not only stifles a potentially revealing philosophical discussion on the nature of scientific investigation but it places the social sciences outside the domain of legitimate science. The textbook's authors might just as well have included

Rutherford's own answer to that question: Science is physics, everything else is postage stamp collecting.

FAILURES OF THE SCIENCE CURRICULA

The major failures of the new science curricula have been noted here: failure to lead students to comprehend the nature of science and technology; failure to give serious consideration to the social effects of science and technology; and failure to include unambiguously the social sciences within the limits of scientific activity. One could argue that these failures should be corrected in order to insure the full intellectual development of the students. Instead, I will propose other, practical, reasons why these inadequacies must be remedied.

The students in the secondary school science courses of today will be the voting American citizens of the future. Yet the new courses are not preparing our youth to recognize and to assess critically those social and political issues having major scientific and technological components. The programs have failed precisely in the areas that are crucial for a broad and intelligent understanding of science and technology within a social context. They are failing, therefore, to educate the American people regarding the realities and problems of modern society.

THE ISSUES DEFINED

Sixty million Americans in 3,000 cities and towns drink fluoridated water, yet there are some large cities—Boston, Los Angeles, San Antonio—where voters have rejected the use of fluorides in the public water systems and still other municipalities where fluoridation is a persistently controversial issue. Each time fluoridation is placed on the ballot the pro- and anti-fluoridationists begin their battle. More often than not, victory is achieved by means of mass persuasion and demagogic tactics. Radical rightists and religious cultists fight fluoridation with a stable of "scientific experts" and adverse testimony, while the pro-fluoridationists are not above resorting to advertising campaigns that forgo rational arguments, display the credentials of their own group of expert supporters, and urge the citizen to vote for fluoridation, without discussion or under-

standing, because it is a gift of modern science.

In this case, the perplexed voter is not likely to have had much help from his high school biology course, even if it was of the BSCS variety. What he needs in order to make an intelligent choice is not more biological knowledge but a better comprehension of the nature of science and its social environment. The voter should have studied the provisional nature of scientific knowledge and its limitations; he should have learned to recognize what constitutes authority in a scientific discussion and to realize that scientific expertise does not necessarily qualify one to be an unbiased judge on social and political issues; and he should have been taught what kind of social behavior accompanies the presentation of technical matter before the voting public.

The fluoridation problem is closely related to the issue of smoking and cancer. Both of them ultimately rely upon statistical evidence and hence raise fundamental questions concerning the propriety of using statistics in place of experiments as the basis of a scientific theory. The 1964 report of the Surgeon General's Advisory Committee on Smoking and Health failed to spur the enactment of regulatory legislation because it unwittingly opened a debate on the nature of science that overshadowed the report's substance and conclusions. Congressmen, along with their constituents, were not satisfied with the advisory committee's contention that there was a statistical association between smoking and cancer. The laymen had been led to believe that scientific theories were properly constructed of immutable causal links, determined by laboratory experimentation, and not built on the shifting sands of statistics. Since they were not told that smoking caused cancer they concluded that more research, not more legislation, was in order. (Sanford A. Lakoff, ed., *Knowledge and Power*, Free Press, 1966.) The solution of many other public health and social problems that rest upon statistical relationships will be jeopardized by this misapprehension unless we are able to educate the public to a more sophisticated understanding of the nature of scientific activity.

The medical effects of polluted air

are established on a statistical basis and therefore any discussion of air pollution will likely end in the confusion noted above. Polluted air, however, is part of the larger problem of environmental pollution and we are slowly realizing that pollution is the price we must pay for the benefits of an industrial civilization. Given this situation, citizens should be educated to weigh intelligently the advantages and disadvantages of our expanding technologies and to determine what level of pollution they are willing to tolerate. This social decision requires that the individual have some insight into the nature of technology and an appreciation of the complex relationship between technological advance and the growth of the economy.

UNDERSTANDING SPACE GOALS

With the completion of the Apollo moon project in the seventies the American people will be called upon to reassess the nation's space goals. We are certainly not preparing our youth for that challenge. What is needed is a citizenry sufficiently aware of the nature of scientific and technological activity so that they will not lend their support to spectacular, and costly, engineering ventures on the mistaken assumption that basic scientific research is being advanced. If national prestige or military necessity dictates the need for planetary exploration then the American people should be discriminating enough to realize, once again, that purposes other than science are being served. Finally, they should be in a position to evaluate the claim that space technology makes substantial and immediate con-

tributions to our economic well being and then act accordingly.

The reformed science curricula offer little that is useful in the area of "space education" and the Nasa-supported educational program—*Introducing Children to Space: The Lincoln Plan* (GPO, 1966)—is unsuitable as preparation for an unbiased review of our space activities. At five years the Nasa-educated child is reciting from a chart inscribed:

I want to go to the moon:

1. To explore
2. To see craters
3. To live there

By seven he is dramatizing the launching of Telstar (language arts), drawing pictures of space vehicles (art), and checking the health of the class to see if his schoolmates could become astronauts (health). At maturity level 11 he is building models of rockets, writing the diary of a space pilot as well as the history of the Boeing Company, geographically locating satellite tracking stations, and, after seven years of indoctrination, "debating the wisdom of spending so much money on space research."

This list of major social and political issues with scientific and technological components could be expanded to include: automobile safety; drug control—from manufacture to use; urban affairs; pesticides; and disarmament—political and technical factors are intertwined here. None of these issues can be met intelligently by a public that is educated largely with scientific facts, ignorant of modern technology, and prejudiced against the social sciences. We cannot make science policy experts of our high

RESISTANCE TO CHANGE

"In many countries university traditionalism and organizational rigidity are so strong as to make it difficult for their institutions to change in such a way as to accept the ever greater proportion of the population deserving higher education and to provide them with the variety and appropriateness of instruction that this expansion will demand. The universities, through their contributions to knowledge, have been the main source from which innovation has sprung, not only in technology, but in a deep social sense, yet in many countries the universities as organisms seem to be more resistant to change than almost any other part of the social framework."

—Alexander King, Director for Scientific Affairs, Organization for Economic Cooperation and Development, Paris, France; Presented to the Committee on Science and Astronautics, January 24, 1968

school students but we can at least train them to take a more critical attitude toward the scientific enterprise and to understand that science and technology have social ramifications. Science education is too important to be left in the hands of scientists and teachers who hold a narrow view of science and are not concerned with its social implications.

REMEDIES

The inadequacies of current science curricula can be partially remedied by implementing the following program.

First, it is important that science educators recognize the failures outlined here. Virtually all of the new science curricula contain policy statements about the importance of seeing science as an open-ended process of discovery, and not as a closed system of truths, but the significance of this is lost as the course proceeds and greater emphasis is placed on the accumulation of factual matter. Similarly, one can find statements about science and its impact on the modern world. It has become customary to write about the terror and power of nuclear weapons, the wonderful possibilities of controlled nuclear energy, and the grandeur of space exploration. These responses, however, are entirely inadequate to the challenges we face; it is my position that the neglected topics be given a more prominent place in science courses and not relegated to brief introductory paragraphs in a textbook or to occasional outside reading assignments.

Second, it will be necessary to expand the training of science teachers to include the required study of the history, philosophy, and sociology of science. The history of science is not to be studied in order to provide a chronological background, or biographical and anecdotal material for the science courses to be taught; nor is the philosophy of science to be explored in search of the minutiae of philosophical arguments. Rather, the combined effect of these three disciplines should be to enlarge the teacher's understanding of science, to force him to look beyond the limits of his specialty and see the wider implications of science and technology.

Third, science programs should be altered to include the neglected aspects of science and technology. I am

not advocating that these aspects completely dominate existing science courses. Rather, I believe they can be integrated with the subject matter of such courses. The atomic theory, for example, has long been associated with philosophical problems relating to the nature of physical reality, problems that have been a direct bearing on our conception of the scope and effectiveness of scientific inquiry. The social significance of this theory is of recent origin and better known to students who are vaguely aware of the potential dangers of nuclear energy and radiation. In this case it would be both natural and easy to prepare a unit on atomism that focuses on physics and chemistry but also explores the philosophical background and social consequences of the atomic age.

Moving to the biological sciences, we encounter the theory of organic evolution. Here the philosophy of science might be used to clear up a confusion that exists in the mind of the general public and is exploited by the anti-evolutionists: Since evolution is a theory and not a fact why should we accept it over any other theory that purports to explain the organic world? Continuing in biology, the student's knowledge of genetics could be expanded to include an investigation of Lysenkoism—its causes and place in communist ideology and Soviet society—as well as the social and moral questions raised by recent advances in molecular biology. Finally, the study of ecology might be joined to a full-scale exploration of environment pollution so that the student would learn that the purely scientific and technical aspects of pollution are trivial when compared to the social, political, and economic ones.

These few examples should suffice to show that I am not calling for a general education, or "science and Western civilization," course for secondary schools. It is current science that concerns me—its social and epistemological goals, and its relationship to technology and the economy. A teacher educated to see science and technology in a broad scope could fully reveal the contemporary scientific scene to his students by teaching the integrated course I have proposed.

Such integration has not yet occurred despite assertions of some science educators and curriculum reform-

ers to the contrary. What we need is a revised science curriculum created by men who are fully aware of the inadequacies I have noted, familiar with the practical problems of secondary school teaching, and willing to subordinate their interest in the informational content of a science course to the pressing demands posed by our industrial civilization. Research for this curriculum might very well be supported by one of the recently created institutes dedicated to the study of technology and society, and to the study of science and human affairs.

Some critics, no doubt, will question the appropriateness of introducing these topics into secondary schools. But we cannot wait until the college level to present this material because 50 per cent of American high school students do not go on to college; if we wish to reach these potential voters we must do so while they are still being educated. And, it is even debatable whether a majority of those in higher education are ever made to understand the nature and social repercussions of science and technology. But that implies an evaluation of science curricula in our colleges and universities and is beyond the scope of this article.

Another criticism, pertinent to the third part of my proposed program, is that these topics should be handled by the social studies teacher because there is simply no room in secondary school physics, chemistry, and biology courses for new material. The responsibilities of the science teacher cannot be shirked so easily. The nature and social impact of science should be taught in conjunction with the major elements of a science course by an individual trained narrowly in a scientific specialty and broadly in the meaning of science and technology. Such an educational experience is unique to the science teacher and only he can present the full face of science to his students. Furthermore, if it is true that there is now no place for new topics then we will be forced to add them at the expense of portions of existing science courses. After all, what is more important: teaching students a little more physics, chemistry, biology, or geology, or preparing them for enlightened participation in a world increasingly dominated by science and technology?

SCIENCE AND PUBLIC POLICY MEETING AT AAAS

The following was specially prepared for the Bulletin by Eugene Skolnikoff, who was organizer of the AAAS meetings being described.

On the occasion of the annual meetings of the American Association for the Advancement of Science (AAAS) in New York City, December 1967, a special series of symposia was arranged under the general title of "science and public policy." The primary purpose of the series was to bring together, for the first time, American university faculty engaged in academic programs in the field of science and public policy.

The impetus for the special meetings came, in large measure, from Dean Don K. Price of Harvard University, the President of the AAAS for 1967 and one of the central figures in the development of the field in the United States; Dael Wolfe, Executive Secretary of the organization, who has been interested in the development of the field for many years; and Eugene B. Skolnikoff, Secretary of Section K of AAAS (social and economic sciences) and head of the Massachusetts Institute of Technology's teaching and research program in science and public policy. Professor Skolnikoff organized the meetings. In preparation for the meetings he surveyed American universities to locate programs in the field of science and public policy and to describe the orientation and structure of these programs.

Some forty universities in the United States were identified as having made a commitment in one form or

other to developing teaching and/or research programs in science and public policy. Many of these had but one "course" in the subject, but a substantial number had rather large-sized research programs or offered a graduate degree with specialization in the subject. In most of the latter institutions, the degree program was located in the political science or government department. A number of disciplines including economics, sociology, history of science, law, and engineering, however, were involved in activities in the field. A few universities included their science and public policy interests within large interdisciplinary programs established to examine the impact of science and technology on human affairs.

The events at the meetings were divided into two parts: one devoted to substantive subjects of likely interest to everyone in the field; the other to relatively unstructured sessions designed to discuss problems encountered as the academic programs grow.

The substantive part consisted of two symposia and an address by David Truman, Provost of Columbia University, who was also 1967 Vice-President of the AAAS for Section K. One of the symposia, "Science and Technology as Instruments of Policy," was arranged by Sanford Lakoff of the State University of New York, Stony Brook. In it, Mr. Fred S. Hoffman, Assistant Director of the Bureau of the Budget, offered a paper entitled "Systems Analysis and Social Policy" in which he discussed the new ap-

proaches to decisionmaking for science and technology in government growing out of the systems analysis developments in the Department of Defense.

The second symposium, "Science, Technology, and Political Decision-making," was arranged by Charles V. Kidd of the Office of Science and Technology. In this session Emmanuel Mesthene, Executive Director of the Harvard University Program on Technology and Society, presented a paper on "Technology and the Polity" and Herbert Roback of the Subcommittee on Military Operations, House Committee on Government Operations, presented a paper on "The Role of Congress." Dr. Truman's address on the general subject of the role of the social sciences in government was entitled "The Social Sciences: Maturity, Relevance, and the Problem of Training."

The unstructured portions of the series included a dinner meeting addressed by Professor Skolnikoff and Dr. Charles Falk, Associate Director of the National Science Foundation; and a workshop session that focused on developments in the university study of science and government relationships. Among the topics discussed at the workshop were the priorities in the selection of research projects, the relations that need to be established with natural scientists, the problems of starting new programs in science and public policy, curricula development, and existing and potential programs for encouraging personnel exchange between government agencies and the universities. In addition the workshop participants explored the utility of a number of devices such as new journals, bibliographic services, and organizational forms that might be established to facilitate increased progress in the field.

The desire for continued communications on these and similar topics was quite strong among the participants. An ad hoc group to be chaired by Professor Skolnikoff was formed to develop a set of alternative arrangements that could be used to maintain contact among persons interested in science and public policy problems. These alternative arrangements are to be discussed at a second workshop tentatively scheduled for December 1968.

SCIENCE AND PUBLIC POLICY ACTIVITIES IN UNIVERSITIES

CHARLES E. FALK

The author is planning director of the National Science Foundation. The following was first presented at the special meeting on Science and Public Policy of the American Association for the Advancement of Science, December 27, 1967.

Science and technology represent quite ancient human endeavors, as illustrated by the invention of the wheel and Egyptian astronomy. What is novel is that these activities have recently experienced a very rapid growth accompanied by unprecedented changes in complexity. It has only been a short time—really not until after World War II—that we actually recognized science as a major national resource. Furthermore, with the very rapid scientific and technological advances of the last 30 years, we literally did not have time to develop a very sophisticated science policy and planning apparatus nor suitable methodologies. It must also be recognized that the incredible increases in our national research and development investments—both government and private—involving a growth of 400 per cent between 1953 and 1966, made some questions, such as those of resource allocations, seem of relatively lesser importance. Since the early 1960s the funding situation has changed and with the flattening of expenditure levels the question of resource allocation has become increasingly prominent.

It is not surprising that our concern for adequate science policies and science planning has grown very rapidly during the last decades. It is true that in this area we are probably more advanced than any other nation in the world, but then the growth and impact of science and technology has also probably been greater in the U.S. than anywhere else. We are obviously now

trying very hard to deal in an increasingly sophisticated fashion with the myriad of questions and problems which face us.

In a way our situation has been described in a short poem by Piet Hein, who is one of the few twentieth-century renaissance men well versed in art, science, and humanities:

"The road to wisdom?—Well, it's plain and simple to express.
Err and err and err again, but less and less and less."

When discussing the impact and interactions of science and technology with society, we must remember that there are two key words in this concept—science and society. These directly imply that practitioners, teachers, scholars, and students of this field must be hybrids of two major intellectual disciplines. They must be familiar with the natural sciences and knowledgeable in the area of social sciences—their philosophy, mode of operation, content, and applications. This obviously represents a rather large order, and it is clear that few people can be truly expert in both of these fields. The balance between an individual's competence in these two areas of knowledge must depend on his personal inclination and formal education; it is impossible to prescribe what this balance should be.

This merging of two considerably different intellectual areas has advantages and disadvantages which are generally common to all interdisciplinary endeavors, but are probably somewhat more pronounced in this case because of the relatively greater traditional difference between the social and natural sciences. As mentioned previously, the science and public policy expert represents a rare and highly sought after commodity, but he frequently has difficulty in finding a home within

either discipline. Furthermore, since science and technology have only been recognized relatively recently as major national resources, the recognition of the importance of science and public policy programs is even more recent. Because of these and other factors, the field of science and public policy as a professional discipline and related university activities seem to find themselves at the present time in the following situation:

1. Most science and public policy experts in universities and government have not evolved from a formal training in both the natural and social sciences and have generally entered the area of science and public policy relatively late in their professional life.

2. There are relatively few university faculty involved in teaching or research in this field.

3. There are very few interdisciplinary teaching or research efforts in existence and it is not easy to get new ones started.

4. There are very few academic institutions which provide educational programs leading to specific higher degrees for students who choose the field of science and public policy as their career.

5. There are also relatively few academic institutions where courses on science and its interactions with society are made available routinely to all undergraduates.

It thus seems evident that we are not providing our educated citizen with a sufficient background to help him reach an adequate understanding of how science will affect his life, nor are we providing him with the skills for an intelligent examination of public policy issues related to science and technology. We are not providing sufficient means for formal education for those who want to engage in a career of science and public policy. As a consequence, there is not an adequate supply of bright, young people who either want to enter this field or who are adequately prepared to enter it. Finally, we are not utilizing sufficiently one of our greatest intellectual resources—the university—in the development and study of one of the most vital topics of our time. It is, therefore, not surprising that we at the National Science Foundation are concerned about the present situation—and we certainly are not alone in

this concern. We have started several experimental programs which will hopefully provide the stimulation, encouragement, and funds to those in universities who are trying to alleviate the previously mentioned shortcomings.

The education divisions of the NSF have and will continue to support curricula development for graduate courses in this area. Recent examples are grants to the Purdue and Indiana Universities for the development of undergraduate and graduate curricula and programs in the area of science and public policy, and our support of the recent Symposium on "Science and the Human Condition" at the University of Illinois. Our Social Sciences Division has and will continue to support research in the areas of economics, history and philosophy of science, political science, sociology, or any combination of these disciplines as they relate to the question of science interaction with society. As a matter of fact, they have noticed a shortage of good proposals in this specific area of investigation.

The NSF Planning Organization also has a great interest, both as a practitioner in the area of science and public policy and as a user of people trained in this field. We have recently initiated a formal University Science Planning and Policy Program, which supports research and initial planning efforts for integrated programs in this field. Because such activities are relatively novel in academic institutions, the Program is intended to be quite flexible and its scope and purpose are defined rather vaguely. It will support efforts related to the study of the administration and support of scientific activities and of the manner in which these activities impinge on and are affected by the social, political, economic, and cultural structure of society. Grants are being awarded for projects dealing with science planning and science policy issues or with the development of techniques and methodologies required in these fields. Since we hope that we will be able to stimulate the development of both students and faculty through novel and innovative efforts, we have no preconceived prejudices about the type of project supported. We also recognize that a period of years will usually be required for effective development.

A possible program might start with a faculty seminar conducted for the purpose of assembling faculty from various disciplines to survey the general spectrum of problems and potential areas of investigation. Such a seminar can perform the very important function of developing a common understanding of terminologies and concepts, especially those which are in areas of lesser familiarity. The seminar might or might not include graduate students. Subsequently the group might concentrate on a specific problem or a specific area of investigation and initiate coordinated individual research projects which, hopefully, would provide graduate student participation. Courses for general curricula might be developed and ultimately programs leading to formal degrees in the area of science planning and policy.

I would like to re-emphasize that this is only an illustration of a possible approach. There are numerous variations and permutations depending upon the aspirations of a university in this field and upon the stage of development of a program in this area. Some universities intend to go immediately to a full-fledged program such as the one just outlined; others prefer to approach the problem in a slower, more experimental fashion. Another factor which can be quite important in determining a course of action is the rigidity or lack of rigidity of the departmental structure in a specific institution. Since interdisciplinary efforts are generally required, rigidity, or what has sometimes been called the guild structure of university departments, inevitably calls for a slower and more experimental approach. Another possibility is the establishment of "Science and Society Centers" which can establish a recognized focal point on campuses for faculty participation, curricula development, sponsorship of seminars, etc. Then again there might be some completely different approach. What I am trying to emphasize is that we are not wedded to any specific concept and would welcome imaginative and innovative projects and programs.

It should be pointed out that our program is relatively new. It would not be fair for me to neglect to mention that while we have lots of good intentions, our funds are somewhat limited. We are talking at this point of a total program on the order of half a million

AEC AND FREE SCIENTIFIC INQUIRY

Harold Green, Washington attorney, discussed in the May Bulletin the AEC Information Control Regulations (see page 41). Another legal report on the matter was issued by the Committee on Atomic Energy of the Association of the Bar at the City of New York on March 26.

The New York Committee reviewed the revised proposals released for public comment on December 28, 1967, and statements by Commission members at the Industry Conference, AEC, February 21, 1968.

The Report states:

"As a result of these and other changes, the impact of the proposed Regulations on the private community has been considerably softened, particularly in terms of the number of persons who might be subjected to the new controls. The revisions also reflect the Commission's efforts to clarify the restrictions and to establish additional procedures to minimize the vagueness and uncertainties which troubled this Committee in its earlier comments.

"Notwithstanding the significant improvements, however, the fundamental issue of governmental control over private research and the communication or publication of independently developed private data remains unchanged. Particularly in light of the serious constitutional questions which would be raised by the new controls, it is the view of this Committee that the Commission lacks the statutory authority to promulgate the proposed Regulations. Congress has never determined that regulatory controls over independently developed private data are necessary and, if such controls are in fact necessary, we believe it is a decision to be made by the Congress and not by the Commission. Accordingly, we urge that the Commission seek from the Congress a clear statutory mandate to impose such controls as the Commission can demonstrate are required by the common defense and security."

dollars per year. Thus it is clear that we cannot at this time award very large grants. On the other hand, we have also up to now not been flooded with a great number of high quality proposals. We expect that the level of the program and the number of good proposals will grow hand in hand. I should also mention that we recognize that new programs like the ones described require not only several years to mature, but that the academic institutions and faculty members need some assurance of continued support. This factor will be taken into consideration in the administration of the program.

We are frequently asked to suggest study areas which might be especially suited to university programs and which would also contribute to the mainstream of current interest. While I can enumerate such areas, I do so with the full realization that the development of local university programs must be dominated primarily by the interests and areas of competence of the faculty members involved.

First, it might be worthwhile to mention to you certain approaches which do not appear to be very suitable. Thus, frequently attempts are made to review past modes of operation; for example, to trace the ways in which decisions related to science and public policy are made in this country. This is an interesting exercise, but basically constitutes nothing but a review. To be really effective, a research project should go one step further and develop, from analyses of past events and current or projected problems, rational and well-substantiated suggestions for improvements. This is very difficult.

Now, from a positive point of view, there are many areas for which an interdisciplinary university team seems to be in an ideal position to make notable contributions. One area, which is receiving increasing national attention, is technology assessment. There is a general feeling that we could have prevented some of our present social problems which have arisen out of the development of technology—such as environmental pollution, urban congestion, nuclear armament—if one had earlier assessed their full potential impact on society and undertaken at that time steps to prevent potential social problems. This is an area which ob-

viously requires teamwork between natural scientists, engineers, and social scientists. However, a word of caution is required. Somehow, our greater awareness of these problems has raised false hopes that increased attention to them would solve all of our difficulties; such misconceptions are dangerous because they lead to false expectations and subsequent disillusionment. As in the development of solutions to most other problems, it is clear that more can be done but it is also equally clear that we have to develop in an orderly fashion our knowledge of the basic factors involved and our ability to deal with them. It might be pointed out that this area is especially well suited to academic attack since it frequently provides a university with an opportunity to tackle local problems and thus fulfill one of its responsibilities—that of public service.

Another and maybe somewhat related study area is assessment of the role which science has played in the development of the social, economic, political, and cultural welfare of a nation. We seem to know most about the economic impact that science has made but even there we are not yet really in any position to put any quantitative estimates on the value of research and development to an economy. However, it is clear that from now on we, as a nation, will have to ask ourselves repeatedly: how important is science and technology to us? Science and technology are expensive activities and the resources of our country, while great, are limited. Consequently, more science support usually means less support to some other socially desirable undertaking. In order to make these decisions in as meaningful a fashion as possible we should be able to have a considerably better understanding of the role of science in our society and what will be gained or lost if we have more or less science activity. It is clear that a comprehensive and integrated study of this matter would be of great value. Such a study would represent a major undertaking and could probably only be accomplished by having many different specialists look at the problem from different points of view. Thus it may represent an ideal study area for a university center which wants to start a comprehensive program involving faculty from many different disciplines. I

should add that one could also attack this problem piecemeal and analyze only one particular type of science impact. Fewer people would be required for this reduced scope.

Another concern which will be with us for a long time deals with resource allocations and the methodology used to achieve better ones. The tools we possess now are still relatively crude and improvements or novel approaches would be most welcome. The application of systems techniques, for example, has already and will in the future undoubtedly produce more sophisticated methods to handle allocation questions. Again this type of study requires a good interdisciplinary team, though this fact is not always appreciated. The team should consist of persons familiar with various methodologies used in the social sciences; the team should also include technical experts familiar with the content and mode of operation of the natural sciences and technology.

Other problems are in the managerial area where all components of our society—legislative or executive, public and private—are continuously asking whether better means cannot be found of organizing the administration and performance of our vast research and development system. Obviously these problems require rather careful study of the sociology of science, public administration, and the organization of various sectors.

The question of science education and the role of scientific efforts in universities are also of concern, especially to NSF. The more we understand the problems involved and the needs of the system, the better we will be able to make meaningful decisions about future actions by either federal, state, or local governments or by university management.

I have discussed, in a rather sporadic fashion, some areas of investigation which might be especially suitable to university research in the area of science and public policy. However, keep in mind my original admonition that these were only examples, since there are unquestionably many, many other projects that will be of interest to not only the faculty and graduate students but which will also help those who have to make decisions in the area of science policy to make these decisions in a more meaningful fashion.

INTERNATIONAL COMMUNICATION IN THE TWENTIETH CENTURY

S. DILLON RIPLEY

S. Dillon Ripley has been Secretary of the Smithsonian Institute since 1964. He previously served as professor of zoology and biology of Yale University. The following was first presented at the Loeb Student Center, New York University, on October 19, 1967, under the sponsorship of the International Center for Communication Arts and Sciences.

Let me attempt to define the most basic and unglamorous of communication problems; this problem has many names, all, you will note, with a pejorative ring.

In government information and propaganda mills, the problem belongs to what are called the slow media; to the development expert or foreign aid planner, it is called a diffuse activity with no visible end-use; to the science establishment, it cannot be expressed as a factor of anything; to almost everyone, it is a wonderful problem to think about, but strictly in non-priority categories.

What is it? Nothing other than the problem of international exchange of scholars and artists and of the fruits of their work. Some may doubt this is really much of a problem area at all.

I submit that the problem today with international communication among scholars or artists is that we expect too much of it in national policy terms, even as we do not pay it enough attention on its own terms.

What do we mean by "expecting too much in national terms?" Let me point out one very clear case. The Department of State's exchange of persons program, which currently has a budget of approximately \$46 million

and involves some 35,000 exchanges a year, exists for this purpose:

"The exchange program has effectively supported one of the nation's most basic international objectives—of helping support strong free societies able to work together, in mutual trust and understanding, on the grave issues of our time."

This language I have just quoted, which comes from a recent report of the United States Advisory Commission on International Educational and Cultural Affairs, translates into a much more direct and succinct phrase whenever State Department officials must justify the program before Congress or at high interagency councils in the executive branch. During these occasions it comes out that cultural exchange programs exist, among other reasons, "to support the foreign policy of the United States."

This is expecting too much. In the actual administration of the program, it means that undue emphasis is given to changing attitudes about the United States, either by bringing foreign nationals to the United States for an explanation of ourselves, in order to make ourselves more liked on our own home ground, or by sending Americans abroad who, it is hoped, will put our best foot forward. It means, for example, that you will find many more journalists receiving grants than, let us say, historians or scientists concerned with basic research.

And, not to place the entire onus on our colleagues in State, it means that in the exchange programs connected with our foreign aid, specifically those of the Agency for International

Development (AID), you will find far more public officials—ranging from chiefs of police, to whom we impart our experience in riot control so that they may use it back home with subversive groups, to aviation administrators, who are left to gaze in awe upon the complex air traffic structures of our eastern megalopolis—than you will ecologists or demographers, who represent professions in which AID ought to have more than a passing interest.

Or, to cite the supreme example of cultural exchange supposedly in the direct service of foreign policy objectives, you will find that on university campuses it is the student politician, rather than the student, who is becoming a scholar who goes to the great international congresses or gets the red carpet tour. This is something, you will recall, that our National Student Association discovered rather belatedly, to its chagrin.

But let me also point out, again to spare my colleagues in Washington from total blame, that the United States is not alone in the business of trying to change attitudes through what is called cultural or educational exchange. Groups of foreign journalists, student leaders, and union chiefs are not unknown in the USSR, so I think it is fair to say that the East likes to play the game too. The non-aligned or "third force" nations also play it very hard. And even from the lofty vantage point of Colombey des Deux Eglises, it appears that cultural and scholarly ties are sometimes put to the service of political objectives, albeit subtle objectives that are beyond our ken, witness the *drang nach Quebec*, but not to Ottawa.

This is not to say the game is inherent wrong, or that we shouldn't play it. Journalists of the world should get together. Even student politicians may have something to say to each other, although their instructions from the home office may make it rather difficult.

But this game, this business of trying to serve national interests through international cultural exchange, is wrong if it is played at the expense of scholarly exchange, as we have reason to know, because the exposure of counterfeit scholarly communication can cast doubt on real communication among all scholars. The game is also wrong if, as I have said, we expect too

much of it. We will be wrong if we keep looking anxiously for a victor, for there can never be one. We will be wrong even if we try to find out who is playing well, if we try to make a box score in terms of foreign policy successes or a sort of Neilson-rating survey of our international popularity, as the U.S. Information Agency (USIA) once attempted.

This is what we must not let happen. If we become too concerned with playing the game that way, we are bound to neglect more serious business: the international exchange of ideas concerning problems to be faced by this generation, or what the State Department called the grave issues of our time.

Consider the world which I inhabit, the world of museums, of exhibits, and of three dimensional education. Certainly there are many who appreciate the potential of the museum in the communication process. Some may be charting new directions in cognitive studies, or the science that seeks to understand what is most readily assimilated and stored in the human brain and what is not. Others may be interested, as I am, in the fact that motion pictures seem to be superseding objects as the star attraction at world's fairs. If this is indeed happening, it is something we should think seriously about. There must be a place for the "manufact" as well as the manuscript in the learning process, for the object which speaks for itself to men of all countries and cultures needs no words to explain it.

This is a challenging area for research, the world of museums and exhibits, because the museum is the freest and least authoritarian form of communication. It is the school of "open end" education, where the student may pick up or reject what he wants.

But this is also a world where international communications and the exchange of ideas is rather neglected. It may well be, in fact, that the museum is the single most-neglected educational resource in the developing world. Certainly we have barely explored the possibilities of museums and exhibits as an educational force among illiterate and semi-literate societies. It is no exaggeration, in fact, to say that teaching or science museums in the emerging nations are going backward,

not forward. In India and Pakistan, for example, there existed some 105 museums or museum-like institutions in 1936. Today there are fewer than half that number. This is an alarming situation when one considers that from childhood on, from illiteracy up, museum education represents one of the earliest and most dramatic ways to stimulate the human imagination.

We have the problems of expecting too much and at the same time neglecting much in international communications involved in scholarly and cultural exchange. What can we do about them?

In the first place, we can begin by simply lowering our national policy expectations for cultural exchange. This act alone may have the paradoxical effect of serving national interests much better anyway. This paradox was well put by our Secretary of State, Dean Rusk, during the first year of the Peace Corps, when he said:

"The Peace Corps can best serve the foreign policy structure of the United States by not becoming part of it."

This is the philosophy, I submit, that should permeate all our efforts in the international exchange of scholars, artists, and their works. We need governmental support, to be sure, to achieve much of what I have been discussing, but we also need the greatest separation, the maximum degree of autonomy from government. Only in this way can we rid ourselves of the inevitable disappointment that comes from expecting too much and only in this way can we correct some of the imbalances or undue emphases I have mentioned.

But can this philosophy or this doctrine of separation really work?

I can only reply that there is much evidence, both of a negative and positive character, to show that it will. On the negative side, to demonstrate the problems of strong identification with government, rather than separation, let me quote from an article in the education section of the *New York Times* (Sunday, October 14, 1967):

"The International Conference on the World Crisis in Education, held at Williamsburg, Virginia, last week, gave an unintentional demonstration of a fundamental dilemma—education in the underdeveloped countries requires massive help from the United States, but any direct governmental

action is suspect as a threat of American cultural imperialism."

The articles go on to describe the birth of the Conference, which took place during a major address by President Johnson in Hawaii, and the subsequent difficulties in planning it when many world leaders expressed the opinion that governmental sponsorship would produce too many education ministers and other officials who would have to confine themselves to government-approved and, therefore, meaningless or at least innocuous statements. As a result the Conference had to be rescued by a private foundation and immediately the paradox factor I have been trying to define went into effect. Among the 170 delegates from 52 nations, there was a surprisingly high percentage of ministers of education and other officials. They attended, after all, and for once they seem to have communicated. President Johnson attended and had the great good sense and grace to account for his presence by saying that he came "as a private citizen and former school teacher."

On the positive side, it can be shown that in this country's experience the scholarly centers that have been established abroad on an independent and thoroughly non-governmental basis have the longest and most distinguished records for stimulating scholarly communication and, incidentally, for providing the base of cultural relations on which political relations must sometimes depend. I am speaking here of such institutions as the American School of Classical Studies in Athens, founded in the last century, or the American Academy in Rome, established early in this century. These centers unite scholars from different lands, brought together not by the purpose of influencing opinions of the one or the other in narrow nationalistic terms, but by a common desire to study and to understand civilizations that are part of the heritage of all men. In the process, they have become almost indispensable to those who would engage in this pursuit. They have continued when political relations have been strained or broken.

The Smithsonian has long sought and is now seeking to foster the growth of such centers. We have had some successes; we have found a

climate of reception in many parts of the world. We attribute these successes in part to the fact that we are not of the government, although we receive help from it, and that we long have maintained a scholarly, independent, and apolitical character. This is the posture that seems to work for us; it seems to overcome all manner of difficulties in keeping the international communications channels open. Let me cite one very recent and rather interesting example. During the last three years the Smithsonian has supported a research center in Cairo and various bi-national archeological research projects or excavations throughout Egypt, from its appropriation of Public Law 480 foreign currencies, which derive from the sales of surplus agricultural commodities. These projects suffered some interruption, of course, during the Arab-Israeli conflict, but a number of them have since been reestablished. Political relations seem to remain at a stalemate, but what I have called the base of cultural relations does in this case continue.

We hope in the future to keep open similar channels, not only in the arts, or archeology, but also in the neglected corners of science. We are particularly concerned with the need to take a biological inventory of the world's natural environments, both marine and terrestrial, in order to understand their relative productivity for the expanding human populations that must someday inhabit or sensibly exploit them. This kind of massive or global inventory will require the strongest international cooperation. It will be a particularly difficult task in scientific relations between the developed and the developing nations, because it is in the latter countries, or in the tropics, that we find the most biologically productive yet least understood of natural environments in the world.

But these tasks do not belong to the Smithsonian alone. If you agree on some of the problems I have sketched, on the philosophy of separation and yet on the need for help from governments in cultural exchange, then we must move forward to implement a recommendation that has appeared in a number of forms in recent years.

It first appeared, I believe, in a study of our cultural relations policies made by Charles Frankel, until re-

cently Assistant Secretary of State for Educational and Cultural Affairs. This study, published by the Brookings Institution under the title *The Neglected Aspect of Foreign Affairs: American Educational and Cultural Policy Abroad*, called for the establishment of a semi-autonomous foundation, able to reunite both federal and private funds, to administer the State Department and perhaps other federal exchange of persons programs.

In a different context and for different purposes, we have since had the Katzenbach, Gardner, and Helms report, which sought to confront openly some of the problems created by the CIA disclosures. This report called for the prompt establishment of "a public-private mechanism to provide public funds openly for overseas activities of organizations, which are adjudged deserving, in the national interest, of public support." We might argue with some of this language, but we find again the philosophy of separation accompanied with help from government.

And we have also since had passage of the International Educational Act of 1966, which carries something of the same philosophy, or at least authorizes the Secretary of Health, Education, and Welfare to utilize fully both government and nonprofit private agencies in carrying out the purposes of the Act.

But, as of this date, all of these recommendations or plans remain just that. There has been no change in the administration of federal exchange of persons programs. The public-private mechanism for the United States' manifold overseas interests does not yet exist. And the International Education Act, though passed into law, is hamstringed by lack of appropriations.

Perhaps we may pardon this inaction, at a time when we face such grave domestic and military challenges. Perhaps we must wait to carry out certain of the grand tasks. But there are some steps we can take now. Let me conclude, therefore, with three proposals.

First, if we cannot now provide the massive assistance in international education that seems required of us, we can at least begin by creating international study centers squarely focused on growth and survival through communications. What these centers

should be, where they are located, and what problems they confront should all be sharply defined by an advisory group. One such center, or one example among many, might be an international institute for the study of cultural exchange. I feel this represents a particular need and one of special interest because, if you will permit me yet another paradox, we are paying today a heavy price for the rapid improvements in communications. We are by way of obliterating whole cultures or submerging them indistinguishably into twentieth-century man. We must, therefore, intensify what social scientists like to call urgent anthropological studies, before the lessons to be learned from disappearing cultures and peoples are irretrievably lost. And our modern communications processes are continually feeding the flames of what has been called the revolution of rising expectations. It is a worldwide revolution, now burning white-hot, and we must study the ways to mitigate its effects.

Second, if we cannot yet build a comprehensive semi-autonomous foundation to administer and coordinate federal and private international exchange programs in the sciences, arts, and humanities, we can at least give greater attention to some of the neglected areas and the institutions that already exist to strengthen them. We know these institutions or mechanisms are productive in terms of scholarship; we know that they build bridges for the exchange of ideas; and we know that they transcend political differences. So let government and private foundations take another look at them and devise ways to support them, without turning them to purposes for which they are not intended and can never serve.

Third, and this is more an exhortation than a proposal, I would remind every government, my own included, that there are areas of international communications in which it is often true that less is more, or that the international interests of individual nations are often best served by those who think less about them and go quietly about their real business of scholarship or creativity. These, I often think, are the persons who forge the real and lasting links of international communications.

The following comments were written in response to Marshall Stone's review of *The Autobiography of Bertrand Russell, 1872-1914*, which appeared in the January Bulletin. The author of the first comment is Lawrence La Fave, associate professor of sociology, Indiana State University, Terre Haute; the second author, J. G. Barense, is assistant professor of philosophy at the University of California, Santa Barbara.

It is sadly ironic that, when their emotions get involved, mathematicians can compete successfully with us ordinary mortals in the arena of self contradiction. Apparently such inconsistencies serve the psychic function of permitting the emotional mathematician to unleash his hostility upon the object which threatens him, even when no factual basis for his charge exists.

Not yet have I read Bertrand Russell's 1872-1914 autobiography. Whether I will be disappointed with it, as mathematician Marshall Stone indicates he is, I cannot say. What I can say is that Stone's alleged book review appears as a thinly-veiled attempt at character assassination.

The glaring inconsistencies within the "review" suggest strongly that he is as unsympathetic toward Russell as he accuses Russell of being toward his co-workers. For example, Stone states that "... There are many ... instances of ... egocentricity. ..."

The word egocentricity I understand to mean self-centered. But Russell intended an autobiography, not a biography of Mrs. Whitehead. Perhaps Stone could relate how one would go about writing a non-egocentric autobiography. Stone feels that Russell probably has no claim to greatness other than his contributions to logic, "... and that his contemporary renown as an actor on the stage of politics will not long survive him."

Stone apparently insinuates that for Russell the international military tribunal is merely a game—that Russell is too cold-blooded to be genuinely upset by the atrocities which the U.S. has committed against the Vietnamese people. How then does Stone account for the willingness of cold-blooded, egomaniacal Russell to finance the tribunal's activities largely

from money received from this very autobiography?

Greatness is rare, in this century as with others. Russell, by any reasonable standards, is one of the truly great men of this century. When we encounter true greatness, let us have the humility to admire it, rather than be jealous of it. Isn't it time that the otherwise excellent *Bulletin* stopped throwing stones at Russell?

LAWRENCE LA FAVE

I write to protest the vicious and incompetent review of Russell's *Autobiography* by Marshall H. Stone.

Evidence of incompetence is Stone's statement that after the publication of *Principia Mathematica* "Russell took leave of logic forever." No doubt at this time Russell's interests importantly changed. But did he leave logic forever? Certainly not. In 1919, his *Introduction to Mathematical Philosophy* was published, and in 1926, with Whitehead, the article "Geometry: Part IV. On Non-Euclidean Geometries" was published in the *Encyclopaedia Britannica*.

Evidence of viciousness plus incompetence is Stone's suggestion that Russell "grew envious of Whitehead's far greater eminence as a philosopher and his unassuming wisdom and infinite humanity." While I do not wish to deny Whitehead the latter properties, since it would not in any case be quite clear what I was denying, the implied claim that Whitehead was more eminent as a philosopher is simply absurd. Russell has been critically influential on the development of both of the two important movements in twentieth-century Western philosophy, logical positivism and ordinary language philosophy. And although Whitehead's early philosophy had some influence on American realism, that movement is now justly dead and Whitehead's later philosophy stands alone, influencing nothing, a monument to obscurity and intellectual irresponsibility.

The *Bulletin* owes Russell an apology.

J. G. BARENSE

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Artists in this issue: William Dempster, pages 2, 19, and 45; Rainey Bennett, pages 4 and 13; Marilyn, page 28; Vladimir Rencin, page 32.

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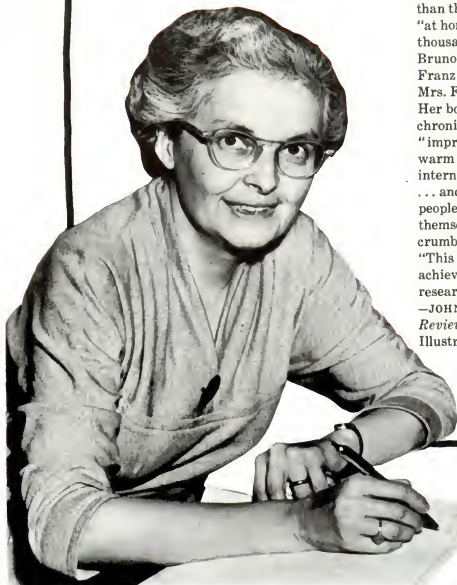


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It was a wave of immense proportions. It brought to our shores talented men and women who were to enrich our culture in every scientific, intellectual, and artistic field. Laura Fermi, whose husband Enrico was among the most distinguished of these, writes of the illustrious Europeans who — driven to America by forces of intolerance and oppression — found not only a haven of comfort but a stimulating home in which they could carry on their work, accomplishing more professionally than they had ever thought possible “at home.” By the tens of thousands they came — Einstein, Bruno Walter, Stravinsky, Chagall, Franz Alexander, Hannah Arendt. Mrs. Fermi knew many of them. Her book is an enthralling chronicle of the tragic or funny “impression of many great and warm personalities, of an international academic community . . . and of brave and determined people who made new lives for themselves after their old lives crumbled.” —*Publishers' Weekly*. “This is a trail-blazing achievement, painstakingly researched and warmly written.” —JOHN BARKHAM, *Saturday Review Syndicate*
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